The American Political Science Review

Vol. 73 March 1979 No. 1

TABLE OF CONTENTS

8963

Page

Pre-Behavioralism in Political Science 9
John C. Wahlke, President, American Political Science Association, 1977–1978

Plus ça change. . . : The New CPS Election Study Panel 32
Philip E. Converse and Gregory B. Markus

Comparisons between Category and Magnitude Scaling of Political Opinion Employing SRC/CPS Items 50
Milton Lodge and Bernard Tursky

Type-Set Politics: Impact of Newspapers on Public Confidence 67
Arthur H. Miller, Edie N. Goldenberg, and Lutz Erbring

Factions in Committees: The U.S. House of Representatives 85
Glenn R. Parker and Suzanne L. Parker

Relative Deprivation Revisited: A Response to Miller, Bolce, and Halligan 103
Faye Crosby

Aristippus in and out of Athens 113
Stephen Taylor Holmes

Comment 129
James H. Nichols, Jr.

Reply 134
Stephen Taylor Holmes

The Good, the Beautiful, and the Useful: Montaigne's Transvaluation of Values 139
David Lewis Schaefer

Metaphor and Political Knowledge 155
Eugene F. Miller

The Intelligence of Stupidity: Understanding Failures in Strategic Warning 171
Steve Chan

Communications 181

Editorial Note 194

Book Reviews 197

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American Political Science Review (ISSN 0003-0554)

BOOK REVIEWS

Political Theory and Methodology 197

Larry L. Adams, *Walter Lippmann.* John H. Hallowell. p. 197.
J. B. Cederblom and William L. Blizek, eds., *Justice and Punishment.* Ernest van den Haag. p. 198.
John P. Clark, *The Philosophical Anarchism of William Godwin.* William G. Nowlin, Jr. p. 199.
Frank M. Coleman, *Hobbes and America: Exploring the Constitutional Foundation.* Richard Allen Chapman. p. 200.
Gray Dorsey, ed., *Equality and Freedom: International and Comparative Jurisprudence, Vol. I, II, & III.* J. Roland Pennock. p. 201.
Ronald Dworkin, *Taking Rights Seriously.* Thomas R. Conrad. p. 203.
H. George Frederickson and Charles R. Wise, eds., *Public Administration and Public Policy.* Kenneth J. Meier. p. 204.
Johan Galtung, *Methodology and Ideology: Theory and Methods of Social Research, Vol. I.* Hayward Alker. p. 205.
George J. Graham, Jr. and Scarlett G. Graham, eds., *Founding Principles of American Government: Two Hundred Years of Democracy on Trial.* Alan P. Grimes. p. 206.
Joseph Haberer, ed., *Science and Technology Policy: Perspectives and Developments.* Boyd R. Keenan. p. 208.
Sandor Halebsky, *Mass Society and Political Conflict: Towards a Reconstruction of Theory.* Peter Bachrach. p. 210.
John C. Harsanyi, *Rational Behavior and Bargaining Equilibrium in Games and Social Situations.* Steven J. Brams. p. 210.
Anthony Heath, *Rational Choice and Social Exchange: A Critique of Exchange Theory.* Richard M. Merelman. p. 212.
Robert H. Horwitz, *The Moral Foundations of the American Republic.* Alan P. Grimes. p. 206.
Irving L. Janis and Leon Mann, *Decision Making: A Psychological Analysis of Conflict, Choice, and Commitment.* Andrew K. Semmel. p. 213.
Douglas Kellner, ed., *Karl Korsch: Revolutionary Theory.* Robert A. Fenn. p. 214.
George Armstrong Kelly, *Hegel's Retreat from Eleusis: Studies in Political Thought.* Patrick Riley. p. 215.
Ernesto Laclau, *Politics and Ideology in Marxist Theory: Capitalism–Fascism–Populism.* Richard J. Boris. p. 216.
Ramon M. Lemos, *Rousseau's Political Philosophy: An Exposition and Interpretation.* Patrick Riley. p. 218.
C. B. Macpherson, *The Life and Times of Liberal Democracy.* Henry S. Kariel. p. 219.
Dwaine Marvick, ed., *Harold D. Lasswell on Political Sociology.* James A. Robinson. p. 220.
A. V. Miller, trans., *Hegel's Phenomenology of Spirit.* Horst Mewes. p. 221.
Francisco José Moreno, *Between Faith and Reason: An Approach to Individual and Social Psychology.* Tracy B. Strong. p. 221.
J. Roland Pennock and John W. Chapman, eds., *Due Process.* Eric Mack. p. 222.
D. D. Raphael, *Hobbes: Morals and Politics.* Richard Allen Chapman. p. 200.
Martin Seliger, *The Marxist Conception of Ideology: A Critical Essay.* Bhikhu Parekh. p. 223.
Joseph Tussman, *Government and the Mind.* Paul F. Kress. p. 224.
Ernest van den Haag, *Punishing Criminals: Concerning a Very Old and Painful Question.* Alan Wertheimer. p. 226.
Vernon Van Dyke, ed., *Teaching Political Science: The Professor and the Polity.* Albert Somit. p. 227.

American Politics 228

Jeffrey M. Berry, *Lobbying for the People: The Political Behavior of Public Interest Groups.* Paul A. Dawson. p. 228.
Richard D. Bingham with Thomas P. McNaught, *The Adoption of Innovation by Local Government.* Robert K. Yin. p. 229.
Forrest P. Chisman, *Attitude Psychology and the Study of Public Opinion.* Samuel A. Kirkpatrick. p. 230.
Irene Diamond, *Sex Roles in the State House.* Barbara G. Farah. p. 231.
G. William Domhoff, *Who Really Rules? New Haven and Community Power Reexamined.* Clarence N. Stone. p. 233.
Robert J. Donovan, *Conflict and Crisis: The Presidency of Harry S. Truman, 1945–1948.* Harold F. Gosnell. p. 234.
James Eisenstein and Herbert Jacob, *Felony Justice: An Organizational Analysis of Criminal Courts.* Jay A. Sigler. p. 235.
David E. Ervin, James B. Fitch, R. Kenneth Godwin, W. Bruce Shepard, and Herbert H. Stoevener, *Land Use Control: Evaluating Economic and Political Effects.* Paul Sabatier and S. I. Schwartz. p. 236.
Dall W. Forsythe, *Taxation and Political Change in the Young Nation, 1781–1833.* Benjamin Ginsberg. p. 237

Richard Y. Funston, *Constitutional Counterrevolution? The Warren Court and the Burger Court: Judicial Policy Making in Modern America.* C. Herman Pritchett. p. 238.
Marianne Githens and Jewel L. Prestage, eds., *A Portrait of Marginality: The Political Behavior of the American Woman.* Jane S. Jaquette. p. 239.
Fred I. Greenstein, Larry Berman and Alvin S. Felzenberg, with Doris Lidtke, *Evolution of the Modern Presidency: A Bibliographical Survey.* Harvey C. Mansfield, Sr. p. 241.
Ernest S. Griffith, *The American Presidency: The Dilemmas of Shared Power and Divided Government.* Elmer E. Cornwell, Jr. p. 242.
Kirsten A. Grønbjerg, *Mass Society and the Extension of Welfare, 1960–1970.* Richard Winters. p. 243.
Hugh Heclo, *Studying the Presidency: Results, Needs, and Resources.* Harvey C. Mansfield, Sr. p. 241.
Morton J. Horwitz, *The Transformation of American Law, 1780–1860.* A. E. Keir Nash. p. 244.
Gary J. Jacobsohn, *Pragmatism, Statesmanship, and the Supreme Court.* Sylvia Snowiss. p. 245.
Morton Keller, *Affairs of State: Public Life in Late Nineteenth Century America.* Stephen Skowronek. p. 246.
Philip B. Kurland, *Watergate and the Constitution.* Henry J. Abraham. p. 248.
Betsy Levin and Willis D. Hawley, eds., *The Courts, Social Science, and School Desegregation.* Charles S. Bullock III. p. 249.
Vincent L. Marando and Robert D. Thomas, *The Forgotten Governments: County Commissioners as Policy Makers.* Alan J. Wyner. p. 250.
Kathleen McCourt, *Working-Class Women and Grass-Roots Politics.* Marjorie Lansing. p. 251.
Stuart S. Nagel, ed., *Modeling the Criminal Justice System.* Elinor Ostrom. p. 252.
Aleksandr S. Nikiforov, ed., *Gosudarstvennyi stroi SShA (The State System of the USA).* Richard M. Mills. p. 253.
Elinor Ostrom, ed., *The Delivery of Urban Services: Outcome of Change.* Robert L. Lineberry. p. 253.
Robert L. Peabody, *Leadership in Congress: Stability, Succession, and Change.* Garrison Nelson. p. 254.
Gerald M. Pomper with colleagues, *The Election of 1976: Reports and Interpretations.* Howard L. Reiter. p. 256.
Stanley Allen Renshon, ed., *Handbook of Political Socialization: Theory and Research.* Kent L. Tedin. p. 257.
Steven R. Schlesinger, *Exclusionary Injustice: The Problem of Illegally Obtained Evidence.* James L. Gibson. p. 258.
Charles L. Schultze, *The Public Use of Private Interest.* Greg J. Protasel. p. 259.
Alan Stone, *Economic Regulation and the Public Interest: The Federal Trade Commission in Theory and Practice.* Paul J. Halpern. p. 260.
G. Alan Tarr, *Judicial Impact and State Supreme Courts.* Robert V. Stover. p. 262.
Stephen L. Wasby, Anthony A. D'Amato, and Rosemary Metrailer, *Desegregation from Brown to Alexander: An Exploration of Supreme Court Strategies.* Gary Orfield. p. 263.

Comparative Politics 264

Don Aitkin, *Stability and Change in Australian Politics.* Lawrence C. Mayer. p. 264.
G. Pope Atkins, *Latin America in the International Political System.* Carlos A. Astiz. p. 265.
Abdo I. Baaklini and James J. Heaphey, eds., *Comparative Legislative Reforms and Innovations.* Alan Rosenthal. p. 266.
Heinrich Best and Reinhard Mann, eds., *Quantitative Methoden in der historisch-sozialwissenschaftlichen Forschung.* Peter H. Merkl. p. 267.
Wolfgang Bick, Paul J. Müller, and Herbert Reinke, *Quantum Dokumentation: Quantitative Historsiche Forschung 1977.* Peter H. Merkl. p. 267.
Ian Budge and Dennis Farlie, *Voting and Party Competition: A Theoretical Critique and Synthesis Applied to Surveys from Ten Democracies.* Benjamin I. Page. p. 269.
Jeffrey Butler, Robert I. Rotberg, and John Adams, *The Black Homelands of South Africa: The Political and Economic Development of Bophuthatswana and KwaZulu.* Richard Dale. p. 271.
Gérard Chaliand, *Revolution in the Third World: Myths and Prospects.* Alexander J. Groth. p. 271.
Brian Crozier with Eric Chou, *The Man Who Lost China: The First Full Biography of Chiang Kai-shek.* Douglas H. Mendel, Jr. p. 273.
Milovan Djilas, *Wartime.* Translated by Michael B. Petrovich. Alex N. Dragnich. p. 273.
Linda L. Dolive, *Electoral Politics at the Local Level in the German Federal Republic.* Arthur B. Gunlicks. p. 274.
Susan Eckstein, *The Poverty of Revolution: The State and the Urban Poor in Mexico.* John A. Booth. p. 275.
Albert F. Eldridge, *Legislatures in Plural Societies: The Search for Cohesion in National Development.* Moshe M. Czudnowski. p. 277.
Milton J. Esman, ed., *Ethnic Conflict in the Western World.* Pierre-Michel Fontaine. p. 279.
John Samuel Fitch, *The Military Coup d'Etat as a Political Process: Ecuador, 1948–1966.* Martin C. Needler. p. 280.
Rainer Hambrecht, *Der Aufstieg der NSDAP in Mittel- und Oberfranken (1925–1933).* Peter H. Merkl. p. 267.

Edward C. Hansen, *Rural Catalonia under the Franco Regime: The Fate of Regional Culture since the Spanish Civil War.* Richard Gunther. p. 281.
Roland J. Hill, *Soviet Political Elites: The Case of Tiraspol.* Joel C. Moses. p. 282.
Michael C. Hudson, *Arab Politics: The Search for Legitimacy.* Ibrahim Abu-Lughod. p. 283.
Ronald Inglehart, *The Silent Revolution: Changing Values and Political Styles Among Western Publics.* Mark Kesselman. p. 284.
R. W. Johnson, *How Long Will South Africa Survive?.* Gwendolen M. Carter. p. 286.
John L. H. Keep, *The Russian Revolution: A Study in Mass Mobilization.* Paul M. Johnson. p. 287.
George A. Kourvetaris and Betty A. Dobratz, eds., *World Perspectives in the Sociology of the Military.* David W. Moore. p. 289.
Leo Kuper, *The Pity of It All: Polarisation of Racial and Ethnic Relations.* Marc Howard Ross. p. 290.
Terry Lacey, *Violence and Politics in Jamaica 1960–70: Internal Security in a Developing Country.* H. L. Nieburg. p. 291.
David D. Laitin, *Politics, Language, and Thought: The Somali Experience.* Crawford Young. p. 292.
Walter Laqueur, *Terrorism.* David C. Rapoport. p. 293.
David K. Leonard, *Reaching the Peasant Farmer: Organization Theory and Practice in Kenya.* Dennis L. Dresang. p. 294.
Arend Lijphart, *Democracy in Plural Societies: A Comparative Exploration.* G. Bingham Powell, Jr. p. 295.
Michael Lipton, *Why Poor People Stay Poor: Urban Bias in World Development.* James W. White. p. 297.
Howard Machin, *The Prefect in French Public Administration.* Ezra N. Suleiman. p. 298.
Alan Marsh, *Protest and Political Consciousness.* Paul R. Abramson. p. 299.
Willy Martinussen, *The Distant Democracy: Social Inequality, Political Resources and Political Influence in Norway.* Lawrence E. Rose. p. 301.
Paul J. Müller, ed., *Die Analyse Prozess-produzierter Daten.* Peter H. Merkl. p. 267.
H. Dean Nielsen, *Tolerating Political Dissent: The Impact of High School Social Climates in the United States and West Germany.* Marilyn B. Hoskin. p. 302.
B. O. Nwabueze, *Judicialism in Commonwealth Africa: The Role of the Courts in Government.* Thomas M. Franck. p. 303.
Helmut Ornauer, Håkan Wiberg, Andrzej Siciński and Johan Galtung, eds., *Images of the World in the Year 2000.* Dennis Pirages. p. 304.
Ergun Ozbudun, *Social Change and Political Participation in Turkey.* Kemal H. Karpat. p. 305.
Arturo Parisi and Gianfranco Pasquino, eds., *Continuità e mutamento elettorale in Italia: Le elezioni del 20 Guigno 1976 e il sistema politico italiano.* Peter Lange. p. 307.
T. J. Pempel, ed., *Policymaking in Contemporary Japan.* Robert E. Ward. p. 308.
Howard R. Penniman, ed., *Italy at the Polls: The Parliamentary Elections of 1976.* Primo Vannicelli. p. 309.
Robert D. Putnam, *The Comparative Study of Political Elites.* Donald D. Searing. p. 310.
Alexander Rabinowitch, *The Bolsheviks Come to Power: The Revolution of 1917 in Petrograd.* Paul M. Johnson. p. 287.
David M. Raddock, *Political Behavior of Adolescents in China: The Cultural Revolution in Kwangchow.* Susan L. Shirk. p. 311.
Bernard Reich, *Quest for Peace: United States-Israel Relations and the Arab-Israeli Conflict.* Richard Cottam. p. 313.
Claude Rivière, *Guinea: The Mobilization of a People.* Translated by Virginia Thompson and Richard Adloff. Dean E. McHenry, Jr. p. 314.
Leo E. Rose, *The Politics of Bhutan.* Joseph W. Dodd. p. 315.
Richard Rose, ed., *The Dynamics of Public Policy: A Comparative Analysis.* Timothy M. Hennessey. p. 316.
Richard J. Samuels, ed., *Political Generations and Political Development.* Marvin Rintala. p. 317.
Wolfgang Schieder, ed., *Faschismus als soziale Bewegung, Deutschland und Italien im Vergleich.* Peter H. Merkl. p. 267.
Dorothy J. Solinger, *Regional Government and Political Integration in Southwest China 1949–1954: A Case Study.* Thomas W. Robinson. p. 318.
Ina Spiegel-Rösing and Derek de Solla Price, eds., *Science, Technology and Society: A Cross-Disciplinary Perspective.* Joseph Haberer. p. 319.
R. Emmett Tyrell, Jr., ed., *The Future That Doesn't Work: Social Democracy's Failures in Britain.* Timothy A. Tilton. p. 320.
Klaus von Beyme, *Gewerkschaften und Arbeitsbeziehungen in kapitalistischen Ländern.* Charles R. Foster. p. 321.
Paul Warwick, *The French Popular Front: A Legislative Analysis.* Howard Rosenthal. p. 321.
Graham K. Wilson, *Special Interests and Policymaking: Agricultural Policies and Politics in Britain and the United States of America, 1956–70.* Hanna J. Cortner. p. 323.
Lawrence Ziring, Ralph Braibanti, and W. Howard Wriggins, eds., *Pakistan: The Long View.* Robert LaPorte, Jr. p. 324.

J. D. Armstrong, *Revolutionary Diplomacy: Chinese Foreign Policy and the United Front Doctrine.* Allen S. Whiting. p. 325.
William J. Barnds, ed., *The Two Koreas in East Asian Affairs.* B. C. Koh. p. 326.
A. Doak Barnett, *China and the Major Powers in East Asia.* Harry Harding. p. 327.
Victor Basiuk, *Technology, World Politics and American Policy.* Henry R. Nau. p. 329.
Coral Bell, *The Diplomacy of Detente: The Kissinger Era.* John Spanier. p. 330.
Maureen R. Berman and Joseph E. Johnson, eds., *Unofficial Diplomats.* David P. Forsythe. p. 331.
Seyom Brown, Nina W. Cornell, Larry L. Fabian, and Edith Brown Weiss, *Regimes for the Ocean, Outer Space, and Weather.* Daniel Cheever. p. 332.
David Carlton and Carlo Schaerf, eds., *Arms Control and Technological Innovation.* Michael J. Sullivan III. p. 333.
Andrew W. Cordier and Max Harrelson, eds., *Public Papers of the Secretaries-General of the United Nations, Volume 7, 1965–1967, U Thant.* Lawrence S. Finkelstein. p. 334.
Herbert Corkran, *Mini-Nations and Macro-Cooperation: The Caribbean and the South Pacific.* Charles E. Pirtle. p. 350.
Arthur Macy Cox, *The Dynamics of Detente: How to End the Arms Race.* Philip A. Odeen. p. 335.
Morris Davis, *Interpreters for Nigeria: The Third World and International Public Relations.* W. A. E. Skurnik. p. 336.
Frank G. Dawson, *Nuclear Power: Development and Management of a Technology.* W. Henry Lambright. p. 337.
Karl W. Deutsch, Bruno Fritsch, Helio Jaguaribe, and Andrei S. Markovits, eds., *Problems of World Modeling: Political and Social Implications.* Harry R. Targ. p. 338.
Annette Baker Fox, *The Politics of Attraction: Four Middle Powers and the United States.* Don Munton. p. 340.
André Gunder Frank, *On Capitalist Underdevelopment.* Robert A. Packenham. p. 341.
David C. Gompert, Michael Mandelbaum, Richard L. Garwin, and John H. Barton, *Nuclear Weapons and World Politics: Alternatives for the Future.* William I. Bacchus. p. 342.
Selig S. Harrison, *China, Oil, and Asia: Conflict Ahead?.* David Mozingo. p. 343.
Michael Hudson, *Global Fracture: The New International Economic Order.* Charles Lipson. p. 345.
Douglas M. Johnston, ed., *Marine Policy and the Coastal Community.* Donald E. Milsten. p. 346.
Victor H. Li, ed., *Law and Politics in China's Foreign Trade.* Henry S. Albinski. p. 347.
Toivo Miljan, *The Reluctant Europeans: The Attitudes of the Nordic Countries towards European Integration.* Glenda G. Rosenthal. p. 348.
Report of the Nuclear Energy Policy Study Group, *Nuclear Power: Issues and Choices.* John H. Barton. p. 349.
Elmer Plischke, *Microstates in World Affairs: Policy Problems and Options.* Charles E. Pirtle. p. 350.
William B. Quandt, *Decade of Decisions: American Policy Toward the Arab-Israeli Conflict, 1967–1976.* Malcolm H. Kerr. p. 352.
Robert A. Scalapino, ed., *The Foreign Policy of Modern Japan.* Daniel I. Okimoto. p. 353.
Oscar Schachter, *Sharing the World's Resources.* John Gerard Ruggie. p. 354.
Richard Smoke, *War: Controlling Escalation.* Richard K. Betts. p. 355.
Stockholm International Peace Research Institute, *Strategic Disarmament, Verification and National Security.* Lawrence V. Gould, Jr. p. 357.
John J. Stremlau, *The International Politics of the Nigerian Civil War, 1967–1970.* L. Gray Cowan. p. 358.

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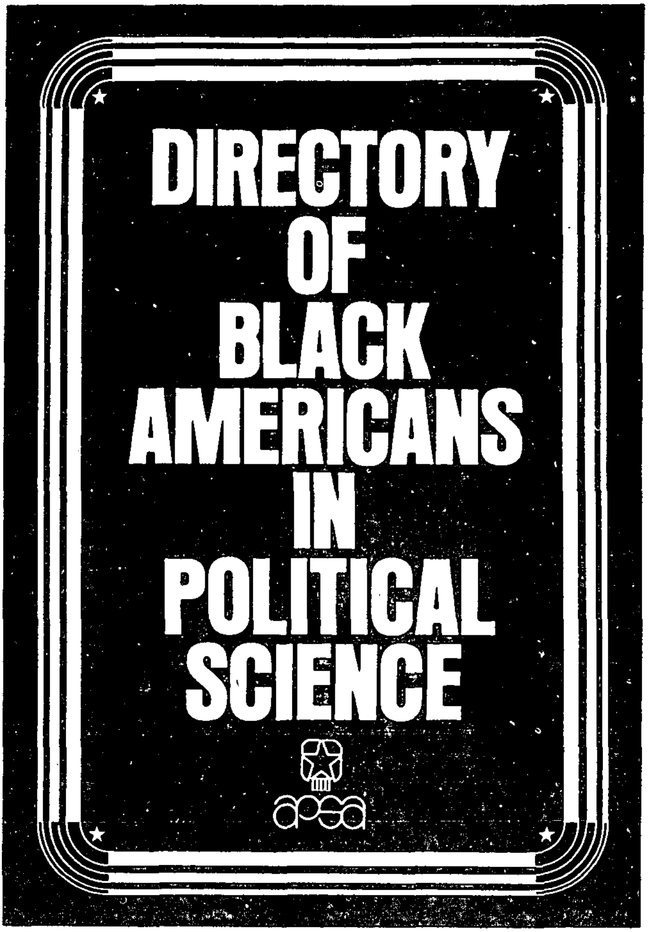

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Figures should be drawn on white paper with India ink and the original tracings or drawings retained by the author (for transmission later to the printer). Copies only should accompany the manuscript. Tables should be typed on a separate page. Insert a guide line, e.g., "Table 1 about here," at the appropriate place in the manuscript.

Books intended for review and all book review correspondence should be sent to Paul A. Beck, Book Review Editor, *American Political Science Review,* Department of Political Science, University of Pittsburgh, Pittsburgh, Pennsylvania 15260. Authors are urged to call their books to the attention of the Book Review Editor to insure that the books will be considered for review.

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Pre-Behavioralism in Political Science

JOHN C. WAHLKE
University of Iowa

Presidential Address, American Political Science Association, 1978

Political behavior research has delivered less than the "behavioral revolution" seemed originally to promise. A survey of recent work suggests that the reason is not its epistemological premises (which are accepted here) or its methodology, but (1) its unsystematic, atheoretical character and limited range of research topics, and (2) the erroneous conception of human nature on which research rests. Compared with either the established principles of modern biobehavioral science or the conceptions of human problems of earlier political science, political behavior research remains "pre-behavioral."

To progress beyond this stage, political scientists must recognize and apply the basic knowledge about human behavior provided by the biobehavioral sciences. Two brief examples of such application are given: how ethological knowledge can supply a needed theoretical perspective for identifying political behavior problems worth studying; and how neurophysiological knowledge, particularly psychophysiology and psychophysics, can correct mistaken conceptions of the relationship between political attitudes, political words, and political actions.

The intellectual history of the so-called "behavioral movement" in political science is by now familiar to all political scientists. How it began in protest against formalistic, historicist, and juristic conceptions of political science's subject matter and rose to presumed hegemony in the discipline against the protests of antibehaviorists of various views needs no retelling here (Truman, 1955; Dahl, 1961; Charlesworth, 1962; Kirkpatrick, 1962; Eulau, 1969a). Also familiar are more recent allegations by radical and "post-behavioral" critics of political behavioralism's biases, "positivistic" blindness to human values, and the "triviality" and "irrelevance" of its findings (McCoy and Playford, 1967; Easton, 1969; Spragens, 1969).

I am concerned here, however, not with the history of political science but with the capability and performance of political behavior research on its own terms. On the one hand, I am convinced that accumulating scientific knowledge of political behavior is a legitimate, worthy, and attainable goal. Yet on the other hand, I am concerned that, although research in political behavior has supposedly captured the main bastions of academic political science, particularly in America, it does not appear to be working toward its goals as effectively as it once promised to do. I therefore accept without further debate here the premises and objectives of "the behavioral persuasion."[1] I wish to discuss here its lack of progress and the reasons for it, and to suggest what political researchers might do about the situation.

I do not wish to imply that all political scientists should become "political behavioralists," or that such study should dominate the discipline. Scientific explanation of political behavior does not take the place of political philosophy, analysis of political values, historical investigation of political events, nonquantitative analysis of empirical phenomena, or many other intellectual undertakings. On the other hand, the fact that political behavior research does not attempt to answer all questions about government and politics does not justify believing that its findings are irrelevant to nonbehavioral political scientists whose major concern is not with the behavioral enterprise. Indeed, the theoretical significance of political behavior research must be measured largely by the relevance of its findings to the concerns of other political scientists—a point to which we shall return in a moment.

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[1] This does not mean that philosophical justification of these premises is unnecessary or that the issues involved in it are easy or trivial. All it means is that other questions are at issue here. A convenient list of the premises and objectives accepted here is the set offered by David Easton (1962, pp. 6–7) as points on which "most students of politics, even those unwilling to accept classification as behavioralists, would probably agree":

1. *Regularities:* the assumption that "there are discoverable uniformities in political behavior."
2. *Verification:* the assumption that "validity of such generalizations about those regularities must be testable, in principle, by reference to relevant behavior."
3. *Techniques:* the assumption that "means for acquiring and interpreting data are problematic and need to be examined self-consciously."
4. *Quantification:* the assumption that measurement and quantification, where possible, relevant, and meaningful, are essential to interpreting data and verifying generalizations.
5. *Values:* the assumption that "ethical evaluation and empirical analysis should be kept analytically distinct."
6. *Systematization:* the assumption that "theory and research are to be seen as closely intertwined parts of a coherent and orderly body of knowledge."
7. *Pure science:* the assumption that "understanding and explanation of political behavior logically precede and provide the basis for efforts to utilize political knowledge in the solution of urgent practical problems of society."
9. [Interdisciplinary] integration: the assumption that "political research can ignore the findings of other disciplines only at the peril of weakening the validity and undermining the generality of its own results."

We are concerned here with only one aspect of the profession, one essential part of our collective task of adding to our stock of knowledge, namely, the empirical study of political behavior. My thesis is that political behavior research to date has worked with deficient and inappropriate concepts, so that it has not yet performed as a "behavioral science" properly speaking. Indeed, judging by its fruits so far, political behavior research might well be called a "pre-behavioral science." But the genuine behavioral sciences do offer principles, findings, and research methods which, applied to the study of political behavior, can contribute to the search for more and better-grounded knowledge.

I

It is necessary first to define a bit more precisely the ground to be covered, for it is widely believed that most modern political science is now "behavioral." This misconception arises from the assumption that "political behavioralism" is identical with "quantification" in political study. To be sure, quantitative measurement and analysis of data is a commonly recognized and important characteristic of behavioral work on politics (see n. 1), but it is by no means its defining or central characteristic. Quantitative methods are appropriate for many nonbehavioral studies and are commonly used in them. What all so-called "behavioral" approaches or studies have in common that *is* definitive of their behavioral character, as Heinz Eulau has cogently explained, is a "commitment ... to the individual person as the empirical unit of analysis" (1963, pp. 13–14, emphasis added).

Even without allowing for misidentification of much work as "behavioral" simply because it is quantitative, however, the predominance of a political-behavior orientation in political science is generally exaggerated, at least regarding quantity. A crude index lumping together what political scientists read along with what they write may be arrived at by classifying all the 4,173 works treated in the book review section of the discipline's major journal over the period 1968–1977 into (1) political behavior works, (2) nonbehavioral quantitative works, and (3) all others. Admittedly, such a classification is highly subjective, and depends in most instances on the assumption that book reviewers report reliably the content of the works reviewed. Still, the proportion of political behavior work is remarkably low, averaging only 4 percent of the total each year, and ranging from a high of 11 percent to a low of 2 percent. Even if we include those empirically oriented and quantitative works (such as nonbehavioral simulations, models, general methodological treatises, etc.) which are often erroneously called "behavioral" solely because of their quantification, the proportion is still not impressive: behavioral-cum-other-quantitative-empirical work averages only 8 percent of the total works each year, with annual proportions ranging from 5 to 17 percent.

A considerably more accurate index of the state of research in political science, however, is provided by articles in *The American Political Science Review* which either report research or deal conceptually, theoretically, or critically with substantive problems for political behavior research. Table 1 shows that, of the 505 articles and research notes published in the *Review* during the period 1968–1977, 180 were political behavior studies, including political behavior methodology, and 325 were nonbehavioral. (The table also shows the particular field of political science addressed by the latter. Those nonbehavioral studies which are quantitative and empirical are included under their respective fields.) Although political behavior research now occupies a much more prominent position than the book review survey suggested—36 percent of the total 505 articles, more than twice as many each year as for any other field—it is still not, by this measure, the majority voice of the discipline. The proportion of the total contributed by political behavior research is about one-third each year, never reaching more than 44 percent in any year and sometimes dropping as low as one-fourth.

Political behavior research, then, measured in this crude way, clearly comprises a minority subset of the discipline. In any case, this sample

Table 1. Subject Matter of *Review* Articles and Research Notes

Subject	1968	1969	1970	1971	1972	1973	1974	1975	1976	1977	Total
Political Behavior	16	22	22	19	16 (2)	15 (1)	17 (2)	14	16 (4)	23	180 (9)
Comparative and Foreign Government, Politics, Institutions, Processes	12	3	11	5	7	6	14	3	5	5	71
Political Philosophy	6	9 (1)	4	5 (1)	7 (3)	2	7	6 (2)	6 (1)	4	56 (8)
Positive Theory	5	4	5	8 (2)	3	3	6 (1)	16 (4)	3	2	55 (7)
American Government, Politics, Institutions, Processes	5	1	3	6 (1)	7	5	13 (3)	6	4	4	54 (4)
Public Policy and Public Administration	2	7	2	3	3 (1)	7 (1)	3	4	3	7 (2)	41 (4)
International Relations, Law, Organization	3	4	1	2	4	3	0	3	2	4	26
General Methodology, Techniques	2	0	1	0	1	3	1	0	2	1	11
Other	1	0	1	2	1	1	2	1	0	2	11
Totals	52	50 (1)	50	50 (4)	49 (6)	45 (2)	63 (6)	53 (6)	41 (5)	52 (2)	505 (32)

Source: American Political Science Review 62–71 (1968–1977).

Note: Numbers in parentheses are the number of "Comments" on specific articles; they have been included in the total figure preceding them.

of 180 articles will serve also as the basis for examining the subject matter and conceptual character of recent political behavior research.

II

In general, individual studies measure up well to high technical standards of research design and conduct. Most political behavior researchers pay careful attention to sampling methods, operationalization of concepts, measurement of variables, testing the validity of findings and inferences, and so on. It is in the aggregate that disquieting features begin to appear.

A. Unsystematic Diversity of Research Questions. For example, these studies collectively span a fairly narrow range of theoretical concerns. A breakdown of the sample of *Review* articles and of a sample of 26 introductory-level textbooks on political behavior (Table 2) clearly bears out the prevailing impression that political behavioralists tend to overspecialize in voting behavior (60 of the 180 *Review* articles deal with it as a principal concern). They also appear much readier to study the behavior (or apathy) of individuals in the mass than that of political activists and members of political elites, even though every political theorist since Plato has recognized that the activity of government consists overwhelmingly of actions by the few (51 *Review* articles deal with elite behavior, compared with 131 on aspects of "mass" behavior). The same breakdown also demonstrates the great extent to which concern with routine, institutionalized behavior overshadows

Table 2. The Subject Matter of Political-Behavior Study: Object(s) of Inquiry and Principal Topics of Discussion in 180 Articles and 26 Books on Political Behavior

Object of Inquiry or Topic	Number of *Review* Articles on Subject	Number of Discussions in 26 Books[a]
Individual Political Behavior in General (Theory, Models, etc.)	3	NA[b]
Mass (Undifferentiated Individuals') Behavior		
Political socialization	14	12
Politicization, apathy, alienation	18	10
Civic obedience, political support	18	7
Political opinions, beliefs, attitudes		
In general (theory, models, etc.)	5	12
Ideology	3	9
Issue opinions, preferences	5	3
Other opinions, beliefs, attitudes	6	2
Total, political opinions, etc.	19	26
Voting behavior	60 (9)[c]	14
Unconventional (non-institutionalized) behavior	2	3
Total, mass behavior	131 (9)[c]	72
Behavior of Political Elites		
General, leadership	7	4
Institutional offices, roles		
Legislative	23	NA[b]
Executive, administrative	6 (1)[c]	NA[b]
Judicial	5	NA[b]
Political party leaders	10	NA[b]
Total, institutional offices, etc.	44 (1)[c]	10
Total, behavior of political elites	51 (1)[c]	14
Methodology	2	NA[b]
Totals	187 (10)[c]	NA

Source: American Political Science Review 62–71 (1968–1977) and 26 selected introductory-level books on political behavior.

[a] Originally reported in Wahlke, 1976.

[b] Not ascertained.

[c] Number exceeds total number of articles surveyed (180) because some articles deal with more than one topic.

concern with unconventional behavior, such as violence or social and political movements. (Only two articles treated these latter topics.)

Even though there are large gaps in what it covers collectively, political behavior research still exhibits an incredible diversity of underlying guiding conceptions, and a near-total lack of consensus about what are the main topics for research, and the main subdivisions of the field. The headings and subheadings in the classification used above (Table 2) were produced by one person's "empirical coding" of the content of the works classified; they are neither used in nor implied by any of the works surveyed. No two of the 26 books surveyed, for example, share more than one major section heading in common. The content, organization, and arrangement of chapters varies equally widely among them. And few of them even deal with more than half the topics listed, broad as they are.

B. Paucity of Theoretical Concerns. A related characteristic of political behavior research in the aggregate is the paucity of its theoretical concerns. It rarely aims at generalization; research efforts have been confined essentially to case studies of single political systems, most of them dealing, as is well known, with the American system (or some part of it). Of our 187 sample discussions of various topics in the *Review* over a ten-year period, 134 were focused entirely on American contexts and 29 on some other single case or system, while only 12 could be considered general or conceptual beyond the single case, and only 10 even ventured comparison among several systems or cases (Table 3).

C. Level-of-Analysis Problems. Lack of generality, top-heavy emphasis on American politics, and insufficient breadth all reflect a more serious theoretical and conceptual deficiency in political research generally, its neglect of the "level-of-analysis" problem. It is sometimes said that political behavior research is "micro-level" study whereas political systems or governments viewed institutionally call for "macro-level" study. But, as Eulau has explained, "Commitment . . . to the individual person as the *empirical* unit of analysis . . . does not mean that research is restricted to the individual person as the *theoretical* focus of investigation" (1963, pp. 13, 14, emphasis added). Roles, groups, cultures, institutions, or other analytic entities may be the theoretical units of analysis even though individuals are the units of observation, i.e., the units about which data are originally collected for later aggregation relevant to the theoretical analysis.[2] The analytic distinction between the study of political behavior of individuals per se and what has been called "the behavioral study of politics" (Eulau, 1962, p. 31) turns on the level of analysis undertaken. The difference between "macro-" and "micro-level" is, of course, not categorical but a matter of degree; along the continuum lie an indefinite number of intermediate levels of generality and complexity (Eulau, 1969b). One essential mark of the theoretical and conceptual adequacy of research is how clearly it specifies its own level(s) of analysis and how clearly and comprehensively it relates its principles and findings to those at other levels.

The point can be simply illustrated. Consider, for example, the frequent studies finding that election results vary predictably with changes in economic conditions (depression, inflation, unemployment, etc.). Insofar as these studies focus on electoral outcomes, using various kinds of aggregate election and demographic data, they clearly operate at a level of analysis closer to the "macro" than the "micro" end of the spectrum. But to ask *why* this relationship obtains is to promote analysis much more "micro" in character, since it involves explaining the voting behavior of individuals. Do their votes reflect personal satisfaction or dissatisfaction with their individual circumstances or with society in general? Both? Neither? What? Plainly, micro-level analysis will expand considerably our understanding of aggregate-level phenomena.

But, by the same token, observation of the macro-level relationship between economic conditions and electoral outcomes ought also make the observer wonder, "So what?" What difference does it make that is of interest to students of politics and government? One might speculate (hypothesize) that sizable gyrations in electoral outcomes over short periods both indicate and promote a certain kind of instability in the system, and thereby contribute toward changing it or bringing it down. One might also suspect that the distribution of material and social resources resulting from government policies which differ according to the different electoral outcomes would vary widely. In any case, questions like these link the original concern over the relationship between economic conditions and electoral results

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[2] The principle being stated here is, of course, what is often described as "methodological individualism" (see Brodbeck, 1968).

Table 3. Level of Generalization in 180 *Review* Articles on Political Behavior

	Number of Analyses											
		Mass (Undifferentiated Individuals' Behavior)										
					Opinions, Attitudes, Beliefs							
Level of Generalization	Individual Behavior (General)	Political Socialization	Politicization, Apathy, Alienation	Civic Obedience, Political Support	Opinion, Belief (General)	Ideology	Issue Opinions, Preferences	Other Opinions, Attitudes	Total, Opinions, Attitudes	Voting Behavior	Unconventional Behavior	Total Mass Behavior
United States Only[a]	0	11	7	14	(10)	(20)	(5)	(3)	11	50	1	94
Other Single Systems	0	1	3	3	(1)	(1)	(–)	(1)	3	10	0	20
Comparative, Multi-system[b]	0	0	7	1	(–)	(–)	(–)	(–)	0	0	0	8
General, Conceptual	3	1	0	0	(3)	(–)	(–)	(2)	5	0	1	7
Vague, Ambiguous, NA	0	1	1	0	(–)	(–)	(–)	(–)	0	0	0	2
Totals	3	14	18	18	(5)	(3)	(5)	(6)	19	60	2	131

	Number of Analyses								
	Political Elites Behavior								
		Institutional Offices or Roles							
Level of Generalization	General Leadership	Legislative	Exec. Admin.	Judicial	Political Party Leaders	Total, Inst'l. Offices	Total Elite Behavior	Methodology	Totals
United States Only[a]	5	(18)	(6)	(5)	(6)	35	40	0	134
Other Single Systems	0	(5)	(–)	(–)	(4)	9	9	0	29
Comparative, Multi-system[b]	2	(–)	(–)	(–)	(–)	0	2	0	10
General, Conceptual	0	(–)	(–)	(–)	(–)	0	0	2	12
Vague, Ambiguous, NA	0	(–)	(–)	(–)	(–)	0	0	0	2
Totals	7	(23)	(6)	(5)	(10)	44	51	2	187[c]

Source: American Political Science Review 62–71 (1968–1977).

[a] Includes studies of single states, regions, counties, or other American sub-systems, and analytic units (parties, institutional groups, etc.).

[b] Includes studies analyzing or comparing more than one American subsystem, as well as foreign systems.

[c] Number of treatments exceeds number of articles (180) since some articles deal with more than one topic.

to concerns at a higher level of analysis, closer still to the "macro" end of the continuum. (For more detailed illustration of this particular substantive problem, see Arcelus and Meltzer, 1975; and Goodman and Kramer, 1975.)

This illustration also suggests another point about levels of political analysis which is almost universally neglected in political behavior research: political inquiry is rooted toward the *"macro"* end of the continuum, since it is there that the common subject matter uniting all political scientists is defined. Traditionally, the initial, orienting curiosity of political scientists, as of political philosophers before them, was about "macro-level" questions concerning properties, attributes, or performances of political systems—stability and change in governments and polities, pursuit and attainment of normative goals (justice, quality, order, etc.), and many others. A *political* researcher can tell what aspects or elements of individual behavior are worth examining only by their ultimate bearing on such matters at higher levels of analysis. What Fred I. Greenstein once said of political scientists studying political socialization applies a fortiori to the student of political behavior more generally (Greenstein, 1970, p. 978):

> He ought to orient himself self-consciously to the ends of [the behavior he is studying]. In "moving back" from the normal preoccupation of political scientists with system-functioning in order to examine the antecedents of behavior in political systems, he needs to be constantly guided by models and conceptions of that from which he is moving back. He cannot merely conceive of his work as an appendage to the "basic" study of human development.

Or, to use Heinz Eulau's terms, only if we are guided by curiosity about "the behavior *of* collectives" of a certain sort do we have any reason to get curious about particular aspects or elements of "the behavior of individuals *in* collectives" (Eulau, 1969b, p. 3).

Such an approach reveals why political behavior research is often criticized for being "too behavioral." We can roughly classify the analyses in our sample of 180 *Review* articles according to the conceptual linkages they make or assume, distinguishing them according to whether or not they include analysis at the political system (or subsystem) level, the level of individual behavior, and/or the level of antecedents or causes of individual behavior. (This distinction is not based purely on level of analysis, since some of the commonly examined antecedents or correlates of individual behavior are in fact contextual or other properties of larger political and social systems, while others, such as psychological factors, are more "micro-level" than the individual behavior they explain. But the logic of explanation in the following discussion makes it convenient to lump both sorts of variables together here.) On this basis (Table 4), we find that only 15 of the 187 analyses in those studies make or suggest linkages between all three of these levels (lines 1–4 in the table); 163 of them totally omit reference to concepts or problems at any analytic level higher than the acting individual (lines 9–10), and, of these, 21 are purely descriptive, relating individual behavior to no antecedents or lower-level concepts or problems (line 10). Five of the others vaguely allude to political-system or political-process concepts or problems without explicitly identifying the higher-level concepts or variables concerned (lines 3, 4, and 6). Three analyses deal exclusively with (nonpolitical) antecedents or causes of individual political behavior without discussing the behavior in question (line 11). And two of them leave vague and unspecified the nature of that behavior (lines 4 and 7).

We can get a more concrete picture by examining the concepts and variables with which these works deal. Table 5 arrays the principal ones (hardly all of them!) in a format matching the logical relationships just described, i.e., grouping together those having to do with system properties or attributes (part A), those defined in terms of individual behavior (part B), and those having to do with causes or antecedents of individual behavior (part C). They have been further grouped within each of these parts more or less according to their location on the "micro-macro-level" continuum. For example, selection of government personnel, or the character of authority and power relations (items 9 and 10 in part A) are in a sense "prior to," i.e., more "micro-level" than, say, political stability or political integration (items 1 and 2 in part A). Similarly, political motivations or ideological orientations (items 9–d and 9–c in part B) are prior to more explicit variables of political role performance (item 1). Despite some inconsistencies, ambiguities, and uncertainties, and allowing for the placement of contextual variables discussed earlier, one can think of Table 5 as displaying the "micro-macro-level" continuum in finer detail, running from item 6 of Table 5–c at the "micro" end to item 1 of Table 5–a at the "macro" end.

Viewed as a whole, this list reinforces the impression just sketched of the unsystematic character of political behavior research. Such system and order as does appear in the grouping and arrangement of entries in the table has been

Table 4. Micro-Macro Linkages in Political Behavior Research: Elements of Explanation and Description in 180 *Review* Articles

Conceptual Linkages[a] Macro ↔ Micro															
					Mass (Undifferentiated Individuals' Behavior)										
								Opinions, Attitudes, Beliefs							
				Individual Behavior (General)	Political Socialization	Politicization, Apathy, Alienation	Civic Obedience, Political Support	Opinion, Belief (General)	Ideology	Issue Opinions, Preferences	Other Opinions, Attitudes	Total, Opinions, Attitudes	Voting Behavior	Unconventional Behavior	Total Mass Behavior
1.	3	2	1	1	0	1	1	(–)	(–)	(–)	(–)	0	5	0	6
2.	3	2	1	0	0	0	0	(–)	(–)	(–)	(–)	0	0	0	0
3.	3	2	1	0	0	0	1	(–)	(1)	(1)	(1)	2	0	0	3
4.	3	2	1	0	0	0	1	(–)	(–)	(–)	(–)	0	0	0	1
5.	3	2		0	0	0	0	(–)	(–)	(–)	(–)	0	0	1	1
6.	3	2		0	0	0	0	(1)	(–)	(–)	(–)	1	0	0	1
7.	3	2		0	0	0	0	(–)	(–)	(–)	(–)	0	0	0	0
8.	3		1	0	0	0	0	(–)	(–)	(–)	(–)		0	0	0
9.		2	1	0	13	17	15	(1)	(2)	(5)	(4)	12	54	1	112
10.		2		2	1	0	0	(1)	(–)	(–)	(1)	2	1	0	5
11.			1	0	0	0	0	(2)	(–)	(–)	(–)	2	0	0	2
Totals				3	14	18	18	(5)	(3)	(5)	(6)	19	60	2	131

Table 4. (continued)

	Conceptual Linkages[a]			Political Elites Behavior								
					Institutional Offices or Roles							
	Macro ←→ Micro			General, Leadership	Legislative	Exec., Admin.	Judicial	Political Party Leaders	Total, Inst'l. Offices	Total Elite Behavior	Method-ology	Totals
1.	3	2	1	2	(1)	(–)	(–)	(–)	1	3	0	10
2.	3	2	1	0	(1)	(–)	(–)	(–)	1	1	0	1
3.	3	2	1	0	(–)	(–)	(–)	(–)	0	0	0	3
4.	3	2	1	0	(–)	(–)	(–)	(–)	0	0	0	1
5.	3	2		0	(1)	(–)	(–)	(1)	2	2	0	3
6.	3	2		0	(–)	(–)	(–)	(–)	0	0	0	1
7.	3	2		0	(–)	(–)	(–)	(1)	1	1	0	1
8.	3		1	0	(–)	(1)	(–)	(–)	1	1	0	1
9.		2	1	5	(14)	(3)	(–)	(7)	24	29	1	142
10.		2		0	(5)	(2)	(5)	(1)	13	13	1	21
11.			1	0	(1)	(–)	(–)	(–)	1	1	0	3
	Totals			7	(23)	(6)	(5)	(10)	44	51	2	187[b]

Source: American Political Science Review 62–71 (1968–1977).

[a] 3 = Political system variables
2 = Individual behavior variables
1 = Variables antecedent to or causal of behavior
Solid-border boxes indicate use of explicit concepts and/or operationalized variables. Dash-line boxes indicate vague, unspecified or merely implied concepts. (Absence of box indicates failure to deal with that level.)

[b] Total is greater than the number of articles (180) because some articles deal with more than one topic.

Table 5. Concepts and Variables Used (or Implied) in 180 *Review* Articles on Political Behavior

A. System Properties, Attributes, Variables (Corresponds roughly to Level 3 in Table 5)	B. Individual Political Behavior (Corresponds roughly to Level 2 in Table 5)	C. Bases and Antecedents of Individual Political Behavior (Corresponds roughly to Level 1 in Table 5)
1. Political Stability: Civil Strife Party change Personnel Circulation	1. Political Role Performance (Elite): Style, power-orientation Roll-call voting Other political decisions: allocation of campaign funds	1. Bases of Conscious Decision, Choice Utility maximization calculations Relative deprivation/gratification Satisfaction with economic situation (past, present, future)
2. Integration of Political Community	2. Political Activism, Ambition (Elite): Office-seeking	2. Identifications, Reference Groups: Race, racial militance Religion Political party Allegiance to dissident groups
3. Political Structure: Composition of political groups Character/level of party competition	3. Non-Institutionalized Behavior: Riot, Protest, Demonstration Participation Demand-making: Complaint, Petition	3. General (Social) Orientations, Attitudes, Predispositions: Tendency toward Conformity/Deviation Social (and political) misanthropy Value priorities Holist/localist conceptions of world
4. Representativeness of Government	4. Civic Role Behavior General Citizen Participation (Scale) Voting: participation candidate choice referenda choices	4. Habit
5. Accountability of Government	5. Political Action Orientations Potential for political violence Potential for riot participation	5. Personality, Character: Sense of personal efficacy Self esteem Achievement need, need inviolacy Group hostility Anomie Social-psychological maladjustment: social inferiority social competence
6. Coalition Formation	6. Political Support: Political trust/distrust Legitimacy orientations Trust/support/approval for regime, authorities, institutions Support for democratic rules Support for reformist action	6. Individual Psychic Developmental Structure/Process: (Phylogenetically fixed) developmental states (Piaget, Kohlberg) Biological pre-programming Schema-processing Spatial archetypes Cognitive balance/dissonance
7. (Policy Outcomes, Consequences)	7. Disengagement (Exit), Apathy, Attention	7. Events (Idiosyncratic): Catastrophe, War Ecological incidents Economic conditions, changes
8. Policy Outputs: Legislative Decisions Administrative-agency decisions Judicial decisions Party platforms	8. Political Communication: Media exposure/usage Information sources Engagement in political discussion	8. Daily-life Experiences: Discrimination Emotional experiences in political groups Marijuana usage Political experience (past)
9. Selection of Government Personnel: Election outcomes	9. Political Cognitions and Perceptions, Cognitive/Affective: a. Political Interest, Knowledge, Awareness b. Evaluations of, Preferences for: Government, Government Effectiveness Authority figures, officials Power, influence, authority Political roles, offices Political parties Candidates Political problems, issues, policies Groups Community sentiment Political events (e.g., riots)	9. Political Context: Form of government (local) Election system, ballot type Party structure: balance, competition Recruitment system: volunteerism Electoral-district type (1-member, e.g.) Prevailing political culture, opinions Workload of government agencies
10. Authority/Power Relationships		
11. Agenda Building		
12. Character of Political Discussions: Level/tone Compromise/bargaining		

Table 5. (continued)

A. System Properties, Attributes, Variables (Corresponds roughly to Level 3 in Table 5)	B. Individual Political Behavior (Corresponds roughly to Level 2 in Table 5)	C. Bases and Antecedents of Individual Political Behavior (Corresponds roughly to Level 1 in Table 5)
	c. Ideological Orientations: Left-right, liberal-conservative Radicalism, extremism Authoritarianism, egalitarianism Political style orientations: generalist/particularist moralist/pragmatist d. Political Motivations, Predispositions: Sense of representation Sense of civic duty Sense of political efficacy Power satisfaction (sense of possession) Political alienation: Powerlessness Normlessness Civic tolerance Policy-thinking modes: Instrumental attitudes (toward politics, government) personal/political good-government/benefits orientation people-helping/community-serving Bandwagon/underdog orientation Friendship preferences (political/nonpolitical)	10. Socioeconomic Context: Community size Organizational development Economic Development: Industrialization Urban-rural balance Econ. dependency Race-, class-structure 11. Family Experiences: Parents' political identity, activity, attention, opinions Authority structure (roles, authority models) Affective relationships 12. (Individual's) Socio-Cultural Background: Sex–Age–Marital status Nation, Region, State, Local community, neighborhood Race–Ethnic group, Tribe Affiliations: Religion, religious attendance Organizational (union, etc.) Friendships, location of friends Education: Level, Type, Political content Social status: Subjective social class Urban-rural-metro origins/residence Income, home ownership Occupation, employment status Mobility: geographic, social

Source: American Political Science Review 62–71 (1968–1977).

imposed by the classifier: few articles deal systematically with more than a handful of the concepts listed. Many deal with them in a different logical fashion from that suggested by the table. And some, in fact, reverse the order of dependence-independence suggested there. The frequent allegation of "lack of relevance" of political behavior research, as well as its relative theoretical immaturity, is borne out by the small number of "end variables" of political inquiry shown in part A of the table, and by the fact (not apparent from the table itself) that these are in almost all instances treated in unoperationalized fashion and not formally recognized as "dependent variables."

The most theoretically significant relationships dealt with in research are those between one or another variable (or small subset of them) from the individual-behavior set (part B of the table) with some one or a few from the causes or antecedents listed in part C. This is appropriate when the objective is to explain individual political behavior. But the choice of variables to be examined (both dependent and independent) rarely relates to any recognized body of "middle-range," let alone more comprehensive theory. Research all too often looks only at relationships between individual-behavior variables (i.e., only those in part B of the table), treating them now as dependent, now as independent, without reference to larger questions or fundamental explanations.

The prevailing atheoretical character of most political behavior research is indicated by the extent to which concepts and variables are no more than brute-empirical operational definitions virtually devoid of conceptual content. Indeed, few bodies of research literature in the social sciences more clearly support the indictment made by several prominent psychologists of "the laissez-faire intellectualism of the operational definition":

> Operational definitionalism ... provided a methodological justification for the scientist not to stray beyond a highly narrow, if reliable base. One could follow a single method in developing data and be "pure," even if this purity were more associated with sterility than virtue (Webb, Campbell, Schwartz, and Sechrest, 1966, p. 172).

D. Preoccupation with Attitudes, Neglect of Behavior. But the most striking and important characteristic of the concepts and variables presented in Table 5, especially those of individual behavior (part b), is that they are not really "behavioral" at all, strictly speaking, but "attitudinal" or "mentalistic." This is most obvious in the case of political cognitions and perceptions (item 9, Table 5–b) which alone make up well over half the entries in the list of individual-behavior variables. It is also true of the concepts of political-action orientation, political support, and disengagement/attention (items 5, 6, and 7, Table 5–b). Of all the concepts and variables used to describe, classify and measure individual behavior, only those under the headings of political role performance, political activism, noninstitutionalized behavior, civic role behavior, and political communication (items 1–4, 8, Table 5–b) are genuinely behavioral. In short, most of the variables of individual political behavior investigated by political behavior research are not conceived in behavioral terms at all, but are defined as entities whose existence and character can only be indirectly inferred, never empirically established. The point has been forcefully stated by Przeworski and Sprague (1971, p. 183):

> The reasoning of a psychometrician is essentially inductive. If a set of observable phenomena is found to covary, it is treated as a function of some underlying trait. The observable behaviors are treated as "items" or "indicators," and an extensive metatheory of error and dimensionality underlies the item analysis approach. Substantive theory enters this kind of measurement only through the back door: . . . The world of a psychometrician is elusive–intelligence, dominance, authoritarianism, efficacy, and fatalism are all terms directly unobservable, somewhat overlapping, and theoretically independent.

Moreover many, if not most, of the variables which appear to be conceived in terms of genuine behavior are in actual research not approached in behavioral terms. Even in voting behavior research, which deals with behavioral dependent variables as much as any field of political behavior study, indexing the individual behavior with direct empirical referents is the rare exception.[3] In many cases what is observed is some aggregate record, such as county voting returns, from which inferences are made to the individuals composing the group, a procedure which, as is well known, can sometimes lead analysts into the trap of fallacious ecological inference.

The most common referent of behavioral as of attitudinal variables, however, is the individual's self-report of actions performed or intended. One expert estimate not long ago was that "some 90 percent of social science research is based upon interview and questionnaires" (Webb, Campbell, Schwartz and Sechrest, 1966, p. 1). In our sample of 180 *Review* articles, 131 of the 187 analyses reported rest partially or entirely on such data (Table 6). Of these 59 are original reports of survey data and 65 report secondary analyses.[4]

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[3] One such exception is a study in which the traces of individuals' ballot marking in a referendum were observed and recorded during the process of counting ballots (Mueller, 1969). In an even rarer type of study, researchers examined administrators' behavior toward militant citizens by actual observation of administrative-agency personnel dealing with their clients (Fainstein and Fainstein, 1976).

[4] As the following table shows, over half the secondary analyses use SRC/CPS data from the Inter-University Consortium for Political and Social Research (38 articles), but a substantial number (15) is based on such other familiar political data sets as those which produced the Almond-Verba (1963) study of *The Civic Culture* or the Verba and Nie (1972) study of participation, or on data from commercial polls (12 articles based on AIPO, NORC, and similar data).

	Number of Articles Using Data for	
Data Sets Used	**Original Analysis**	**Secondary Analysis**
Various SRC/CPS (University of Michigan)	2	38
Verba-Nie Participation Study, 1967	0	1
Butler and Stokes, Great Britain, 1963	0	2
Almond-Verba Five-Nation Study	0	7
SRC/SERS (University of North Carolina)	0	2
SRC High School Seniors/ Parents Study (Jennings and Niemi)	5	0
McClosky Minnesota and U.S. Ideology and		

(continued next page)

Nothing manifests more clearly the "mistaken belief in the operational definition of theoretical terms" mentioned earlier (Webb, Campbell, Schwartz and Sechrest, 1966, p. 3) than this use of survey data. It was, indeed, expressly that which Campbell and his associates had in mind:

> The corkscrew convolutions of the maze of behavior were ironed, by definitional fiat, into a two-dimensional T-maze. To define a social attitude, for example, solely by the character of responses to a list of questionnaire items is eminently legitimate–[but] almost everything we know about attitudes is also suspect because the findings are saturated with the inherent risks of self-report information. One swallow does not make a summer; nor do two "strongly agrees," one "disagree," and an "I don't know" make an attitude or a social value (Webb, Campbell, Schwartz and Sechrest, 1966, p. 172).

E. Mentalistic Conceptions of Attitude. Unfortunately, reliance on verbal self-reports about internal mental processes as well as about overt nonverbal behavior is not simply the accidental by-product of a methodological tactic. It rests on a conception of the behavioral dynamics of human actors borrowed largely, and in oversimple fashion, from cognitive social psychology. Besides the premise of methodological individualism, political behavioralism also generally accepts the Weberian conception of behavior and action according to which the understanding of human behavior, as distinguished from the understanding of behavior of nonhuman animals or inanimate objects, requires the intellectual method which Max Weber called *verstehen* (Weber, 1947). That is, to understand any human action one must know its "meaning" to the actor, i.e., the actor's reasons, rationale, motivations, and cognitive picture of the situation.

But judging from their research, political behavioralists have implicitly added a corollary assumption which is not necessarily part of Weber's viewpoint, that *only* actions which are "meaningful" in this sense are involved in social and political life. *Every* politically relevant action (or failure to act) is assumed to grow out of (be caused by) some self-conscious intellectual act (cognition, emotion, desire, belief, and so on). At the same time it is taken for granted that the individual person is consciously aware of inner mental processes and can directly report on them on suitable occasions. The inner (causal) attitude is by definition isomorphic with the action it effects. The relationship between the verbalization of that inner attitude and the "objective" attitude itself can be assumed to be one-to-one, provided questions are not ambiguously phrased and interviewers are alert to respondents' deliberate attempts to misinform or mislead. And therefore, assuming such care is taken, we can confidently expect the relationship between self-reported attitudes and resulting behavior (or, as is often the case, self-report of that) to be equally isomorphic.

This is, of course, a grossly oversimplified sketch of the situation. The frequent incorporation of Freudian concepts–libidinous drives, superego suppression of ego urges, displaced hostilities, and so on–drastically modifies the picture, but not so much as might first appear. Freudian entities are just as "mentalistic" or unobservable empirically as more surface-level rational opinions and beliefs. Just as psychoanalysts analyze verbal expressions to infer inner feelings, social scientists administer multiple-item scales and subject them to increasingly sophisticated statistical analysis, such as latent structure analysis and multi-dimensional scaling, to elicit verbal responses and infer innermost attitudes, many of them (like authoritarianism, for example) closely related to the province of psychoanalysis. Both procedures assume that a verbal account is faithful to an empirically inaccessible mental state, if correctly interpreted.

Despite rigorous empiricism and methodological individualism, then, and despite minor variations in the particular version of the model

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(footnote 4, continued)

Data Sets Used	Number of Articles Using Data for: Original Analysis	Number of Articles Using Data for: Secondary Analysis
Political Values Study	0	2
Nine-nation Political Elites Study	0	1
Opinion Research Corporation Polls	0	1
AIPO Polls	0	4
NORC Polls	0	4
Great Britain SSRC Polls	0	1
California Polls	0	1
Japan Polls	0	1
Miscellaneous others	52[a]	0
Totals	59[a]	65

Source: American Political Science Review 62–71 (1968–1977).

[a] Two surveys used in two articles each; one survey used in four articles. Total number of surveys used in original analyses other than SRC/CPS and Jennings-Niemi SRC/CPS High School Seniors and Parents Study is therefore 48. Total including these is 55.

Table 6. Research Method (Data Base) in 180 *Review* Articles on Political Behavior

	Number of Analyses											
		Mass (Undifferentiated Individuals' Behavior)										
					Opinions, Attitudes, Beliefs							
Research Method, Data	Individual Behavior (General)	Political Socialization	Politicization, Apathy, Alienation	Civic Obedience Political Support	Opinion, Belief (General)	Ideology	Issue Opinions, Preferences	Other Opinions, Attitudes	Total, Opinions, Attitudes	Voting Behavior	Unconventional Behavior	Total Mass Behavior
1. Experiment	0	0	0	0	(–)	(–)	(–)	(–)	0	3	0	3
2. Experiment + Survey (Secondary)	0	0	0	0	(–)	(–)	(–)	(–)	0	1	0	1
3. Survey: Original Data	0	11	8	9	(–)	(1)	(1)	(3)	5	10	0	43
4. Survey (Orig.) + Roll-Call Data	0	0	0	0	(–)	(–)	(–)	(–)	0	1	0	1
5. Survey (Orig.) + Agg./Doc. Data	0	0	0	0	(–)	(–)	(–)	(–)	0	1	0	1
6. Survey, Secondary: SRC/CPS	0	1	3	5	(1)	(–)	(–)	(–)	1	20	0	30
7. Survey, Secondary: Other	0	1	6	3	(1)	(2)	(4)	(1)	8	0	1	19
8. Survey (Secondary) + Roll-Call	0	0	0	0	(–)	(–)	(–)	(–)	0	5	0	5
9. Survey (Secondary) + Agg./Doc.	0	0	0	0	(–)	(–)	(–)	(–)	0	5	0	5
10. Roll-call Analysis	0	0	0	0	(–)	(–)	(–)	(–)	0	0	0	0
11. Aggregate, Documentary, Other Data	0	0	0	1	(–)	(–)	(–)	(–)	0	14	0	15
12. Non-research, Conceptual, Synthetic, etc.	3	1	1	0	(3)	(–)	(–)	(2)	5	0	1	8
Totals	3	14	18	18	(5)	(3)	(5)	(6)	19	60	2	131

Table 6. (continued)

Research Method, Data	Number of Analyses								
	Political Elites Behavior								
		Institutional Offices or Roles							
	General, Leadership	Legislative	Exec., Admin.	Judicial	Polit. Party Leaders	Total, Inst'l. Offices	Total Elite Behavior	Method-ology	Totals
1. Experiment	0	(–)	(–)	(–)	(–)	0	0	0	3
2. Experiment + Survey	0	(–)	(–)	(–)	(–)	0	0	0	1
3. Survey: Original data	7	(8)	(2)	(–)	(5)	15	22	0	65
4. Survey (Orig.) + Roll-call Data	0	(–)	(–)	(–)	(–)	0	0	0	1
5. Survey (Orig.) + Agg./Doc. Data	0	(–)	(–)	(–)	(–)	0	0	0	1
6. Survey, Secondary: SRC/CPS	0	(–)	(–)	(–)	(1)	1	1	1	32
7. Survey, Secondary: Other	0	(–)	(–)	(–)	(–)	0	0	0	19
8. Survey (Secondary) + Roll-call	0	(2)	(–)	(–)	(–)	2	2	0	7
9. Survey (Secondary) + Agg./Doc.	0	(–)	(–)	(–)	(–)	0	0	0	5
10. Roll-call Analysis	0	(11)	(–)	(4)	(–)	15	15	0	15
11. Aggregate, Documentary, Other Data	0	(2)	(4)	(1)	(4)	11	11	1	27
12. Non-research, Conceptual, Synthetic, etc.	0	(–)	(–)	(–)	(–)	0	0	0	11
Totals	7	(23)	(6)	(5)	(10)	44	51	2	187[a]

Source: American Political Science Review 62–71 (1968–1977).

[a] Number exceeds number of articles (180) since some articles deal with more than one topic.

they use, political behavioralists still work from a dualistic, half-empirical half-metaphysical conception of the human individual, in which the body is governed by inner mental impulses which lead to chosen destinations and goals. "Explanation" of overt political actions, thus envisaged, primarily entails reading the mind to discover action-shaping political attitudes and thoughts. Inadvertently, political behavior research has thus become steadily more preoccupied with explaining the variance in attitudinal dependent variables and has progressively lost sight of the more genuinely behavioral variations which constitute the material essence of politics and government.

Even though the basic concept, "attitude," is ordinarily conceived as a "predisposition" to respond uniformly to a broad range of different stimuli or questions (Campbell, 1963), the level of abstraction of attitudinal variables in research is frequently not much higher than that of the behavioral variables they purport to explain. Voting choices, for example, are explained by candidate image or evaluation, by attitudes towards the candidates' party labels, and so on. Attitudes of political support or nonsupport are expected to explain obedient, rebellious, or subversive activity. Indeed, a former president of the American Association for Public Opinion Research has recently suggested that "what has been found wanting [in explanation of behavior by attitudes] is the way so much attitude research has been conducted with . . . reliance on attitude scales of an *unwarranted high order of generality and abstraction*" (Crespi, 1977, p. 294, emphasis added).

It is no new discovery that the correlation between attitudes and predicted behaviors, even at the lowest levels of abstraction, has ranged in actual research results from negligible to nil. As early as 1934 a classic study by LaPiere reported that the prejudiced attitudes self-reported by several hundred or so innkeepers and restauranteurs were almost invariably contradicted by their seemingly unprejudiced overt behavior towards oriental patrons (LaPiere, 1934). Related problems at a wholly attitudinal level were extensively discussed by Philip Converse in a series of works arguing that many of the issue-opinion and ideological attitudes reported in survey research on American voting behavior were essentially "non-attitudes," in that intercorrelation or "constraint" among particular components of the alleged ideological orientations was too low to predict one from another of them in about 80 percent of the cases (Converse, 1964a, 1964b, 1970). Perhaps the most striking evidence of the inadequacy of attitudes as ordinarily conceived to explain behavior is the still largely neglected study by Stanley Milgram which sought to explain obedience to authoritative directives by asking subjects in an experiment to administer what they believed to be lethal dosages of electric shock to supposed other subjects. Their observed behavior ran spectacularly contrary to norms and attitudes which not only they themselves but also psychiatrists and other observers believed would control their behavior (Milgram, 1974).

Such evidence and such occasional expressions of skeptical concern have had little effect on political behavior research, however. To be sure, Converse's discussion of "non-attitudes" generated a flurry of concern over the nature of opinions on political issues, but the response as often as not has been not more careful investigation of the attitude-behavior relationship but further preoccupation with and reification of the "attitudes" which Converse has called into question (Pierce and Rose, 1974). But beyond that, one finds only the barest recognition of the problem on only the rarest occasions. Only one of the articles in the sample of 180 *Review* articles (Sullivan and O'Connor, 1972) explicitly recognized it as a general problem, going so far as to refer readers to one of the more important works on the subject (Wicker, 1969).

F. Summary. In sum, in spite of the technical and methodological expertise found in individual projects of political behavior research, collectively they exhibit two serious conceptual shortcomings which severely hamper their capacity to obtain theoretically (or practically) significant results. First, they are not anchored in macro-level political theory. That is, research on political behavior is not oriented by an awareness of fundamental questions about the polity or its citizens, nor does it link up findings about individual behavior to any such concerns, whatever the original motivation for the research. Second, they rely on a deficient general behavioral theory, on what earlier political philosophers would call a flawed conception of human nature and modern biobehavioral scientists would call an inadequate and erroneous model of the functioning individual human organism. In both respects, although in different senses, the condition of political behavior research can be accurately described as "pre-behavioral."

III

Remedying these deficiencies calls for more than a little additional random borrowing of a few more terms and ideas from a few more behavioral sciences besides social psychology. It requires surmounting our "biobehavioral illiteracy."[5] That is, it calls for learning in some detail the knowledge about human behavior offered by the "hard science" behavioral disciplines as well as that offered by the "softer" brands of psychology. It is worth emphasizing that what these disciplines have to offer is *knowledge* about human behavior, findings about how the human organism works as valid and reliable as other natural science knowledge about other phenomena. Political scientists ignore that knowledge at their peril; they rest their work on flimsier, less scientific and untestable pseudo-theories and models of human behavior at the risk of talking utter nonsense about political behavior. The people whom political scientists study are, after all, no more exempt from the laws of behavioral dynamics than from the laws of gravity.

The desired relationship between political science and the biobehavioral sciences is analogous to the relationship between astronomy on the one hand and physics and chemistry on the other. Astronomers know that all the relevant laws of physics and chemistry apply to all celestial phenomena just as they do to other physical phenomena. Astronomers do not look to the more basic sciences for suggestive analogies or novel conceptions to titillate their imaginations into creating novel, ad hoc hypotheses and theories uniquely applicable to planets, stars, and galaxies. They look for established principles about the behavior of all matter from which they can logically deduce hypotheses about heavenly bodies in particular. And they also take care in their work to see that none of their assumptions, postulates, or hypotheses, from wherever derived, contradict such basic principles. In the same way, the biobehavioral sciences must be recognized as "basic" to the study of political behavior, and what they tell us about the mechanisms and principles governing human behavior must guide our hypothesizing and theorizing about political behavior.

We cannot spell out here how political scientists might go about applying basic biobehavioral knowledge in their research. But we may get some rough ideas by glancing at some of the tentative steps already being taken in this direction. One is what is commonly called "biopolitics"; the other is the application of psychophysiological and psychophysical concepts and methods to the study of political attitudes now going on primarily in the Laboratory for Behavioral Research in the Department of Political Science at Stony Brook.

Insofar as the term "biopolitics" connotes a special, esoteric subfield of study, it is an unfortunate misnomer. "Biopolitics" is better seen as providing a perspective applicable to the entire field, attempting to view human social and political affairs in the light of Darwinian evolutionary biology. Most writers on "biopolitical" subjects, however, draw more heavily on modern ethology than on evolutionary biology per se.[6]

Although *homo sapiens sapiens* is one of the animal species least studied by ethologists, what they have learned about other species applies to human beings also. Especially important is the principle stated by Konrad Lorenz (1965) in a discussion of Darwin's work:

> ... Darwin was fully aware of a fact which, though simple in itself, is so fundamental to biological behavior study that its rediscovery ... is rightly considered the starting point of ethology.
>
> This fact, which is still ignored by many psychologists, is quite simply that *behavior patterns are just as conservatively and reliably characteristic of species as are the forms* of bones, teeth, or any other bodily *structures*.... That *behavior patterns have an evolution exactly like that of organs* is a fact which entails the recognition of another: that they also have the same sort of heredity (pp. xii–xiii, emphasis added).

Most of us are aware of many examples of the kind of species-characteristic, "pre-programmed" behavior patterns which the ethologists have in mind: the stylized nest-building of Baltimore orioles, the courtship dances of bower birds, the ritualized mate-seeking and domicile-building of beavers, submissive groveling and baring of the throat by dogs or wolves toward their more dominant conspecifics, to name but a few.

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[5] The term is paraphrased from Albert Somit's comments on several occasions lamenting our "biological illiteracy."

[6] The first political science usage of the term was in the title of an article by Lynton K. Caldwell (1964), but the word became familiar only after publication of Thomas Landon Thorson's book of that title (1970). As W. J. M. Mackenzie has noted, the word was actually coined earlier by Morley Roberts (1938), in an organic corporate analogy of the body politic to a biological organism, and then "independently reinvented" later (Mackenzie, 1975, p. 297n). A bibliography of the literature of biopolitics is provided by Peterson (1976) and Somit (1976, pp. 279–330).

Social scientists, however, except for those few identified with "biopolitics," shy away from this principle, refusing to admit its applicability to human behavior. As Niko Tinbergen, the Nobel Prize-winning ethologist observed, criticizing "the almost universal misconception that the causes of man's behavior are qualitatively different from the causes of animal behavior,"

> somehow it is [falsely] assumed that only the lowest building-stones of behavior, such as impulse flow in peripheral nerves, or simple reflexes, can be studied with neurophysiological or, in general, objective methods, while behavior as an integrated expression of man as a whole is the subject matter of psychology. Somehow it is assumed that when, in investigating behavior, one climbs higher and higher in the hierarchical structure, ascending from reflexes or automatisms to locomotion, from there to the higher level of consummatory acts, and to still higher levels, one will meet a kind of barrier bearing the sign, "Not open to objective study; for psychologists only" (Tinbergen, 1969, p. 205).

The reason most commonly offered by social scientists for rejecting this important principle is that the human being is a cultural and not an "instinctual" animal. But this argument dodges the main issue. In the words of Rene DuBos (1968, pp. 40, 68):

> Culture is an expression of man's response to the physical and human environment. These responses take the form of behavioral patterns and emotional relationships as well as the development of utilitarian objects. . . .
>
> Considered broadly, evolution always involves learning from experience. The learning may take place by storage of genetic information by chromosomes, by accumulation of knowledge and skills in the individual organism, or by transmission of practices and wisdom in institutions or in society as a whole.

Political scientists need not argue dogmatically about which patterns of human behavior are uniquely "genetic" and which are uniquely "cultural." They need simply to recognize the inseparable interdependence of both, and to distinguish those cases where people are behaving in ways characteristic of all human organisms acting in similar circumstances from cases in which their behavior is better described as distinctively individual responses, i.e., behavior different from what might be observed or expected of other individuals responding to similar stimuli in similar circumstances. Instead of arguing about learning or inheritance, nature or nurture, genetics or culture, political scientists can more profitably observe political phenomena carefully, identify and describe accurately whatever widespread patterns of behavior they detect, and ask to what extent those patterns embody the kind of species-characteristic, pre-programmed behavior studied by ethologists. In other words, the investigator of political behavior should apply ethological principles first as working hypotheses about fundamental behavioral mechanics, as directives for future research. Ethology is a source of *questions* to be asked, not answers to questions political scientists have not yet formulated, let alone asked. To demand to know, "What great and illuminating truth about mankind follow[s] from our realization of his having evolved?" is, as Sir Peter Medawar (1976) has said, to put the question wrongly:

> It is, indeed, not a grand ethological revelation that the scientist should seek from his awareness of the evolutionary process, but rather an enlargement of the understanding made possible by a new or wider angle of vision, a clue here and an apt analogy there, and a general sense of the evolutionary depth in contexts in which it might otherwise be lacking (pp. 497–98).

As illustration, consider one of the most striking but least examined sets of political behavior phenomena which does suggest the working of the pre-programmed behaviors studied by ethologists: the ubiquity of warfare in human history. First, from an ethological perspective, one must conclude that war is statistically "normal," in that at any given time there is either the actuality or probability of war somewhere in the world. Second, very little of the lethal and violent behavior manifested by those participating in the complex social and political-action pattern we call war is explainable as the result of some "instinct of aggression" peculiar to the individual war-makers, or even of some aggressive urge "pre-programmed" into all people. On the contrary, the evidence is overwhelming that

> the human attributes that underlie organized group violence probably have very little to do with the capacity to get angry at someone. Instead, paradoxically, war depends on the *cooperative, group-bonding, authority-accepting* aspect of human nature. Consequently it is likely that group aggression has very different motivational bases than individual-versus-individual fighting, although the two may overlap in some situations (Alcock, 1978, pp. 24–25).

Several more directly political working hypotheses are suggested by this capsule account. One concerns the nature of the groups at war. To oversimplify, apparently the phenomenon we call war is by definition distinguished from all other forms of violent intergroup human conflict by being a contest between "polities," or "political communities." Such communities (which resemble what some ethologists have called "pseudo-societies," in that they tend to become semi-discrete breeding populations) are distinguished in terms of differences between members' conflict behavior. Within polities, ultimate violence between persons or groups is suppressed or severely inhibited. Against "outsiders," however, not only is it tolerated; during interpolity conflicts it is encouraged, rewarded, and often required. Inconsistent as this may appear ethically or rationally, it appears to be utterly consistent biologically.

Moreover, organization of human life through division into such political communities has apparently always been the universal rule. Although much fruitful research could be done on the relationship of political bonding to other ties, such as individual ties to family, party, and work-group, political bonding seems in some respects fundamental and central to all the others. Although individuals can subsist outside a family, a club, or any limited human group, a single person subsisting normally outside of any polity is almost unknown. This bears out the earliest known ethological working hypothesis expressed in classic form by Aristotle: "Man is a political animal."[7]

Just as the pre-programmed behaviors which constitute membership in *some* polity underlie war, they also underlie government, and in particular the relationship of the polity's members to the persons and symbols which constitute their government. Political order within the community rests not on the members' pre-programmed membership alone, however, but on similarly pre-programmed "authority-accepting behaviors" as well (Alcock, 1978, pp. 24–25). This is to say, as indeed Fred Willhoite (1972) has well said, that

> man's biological nature incorporates propensities to establish and sustain dominance-deference hierarchies within his social groupings, that is, that stratification of political authority, power, and influence may be *by nature* intrinsic to human social existence (p. 1110).

Systematic social science offers no better evidence of the enormous power of these pre-programmed "authority-accepting behaviors" to shape individual action in everyday life than the behavior of subjects in Milgram's study of obedience cited earlier. Varied though the mode of their compliance may have been, *all* of Milgram's subjects manifested authority-accepting behavior, clearly justifying the working hypothesis that their behavior was shaped and determined to an important degree by some sort of "pre-programming."

Though this excursion into the implications of evolutionary biology and ethology has been sketchy, it should show that the utility of these biobehavioral sciences lies first in their forcefully directing the attention of political researchers back to broad and fundamental problems, most of which have been central concerns of traditional political philosophy and of political science in earlier times. The biobehavioral perspective views political phenomena from the standpoint of the entire human species, in the context of its evolutionary history. It thus provides the only justifiable baseline for comparing variations in the behavior of various human groups. Second, it clearly forces us to reexamine our two-dimensional, oversimplified, supercognitive, social-psychological model of the acting human individual. It makes us take into account important uniformities in all human behavior so far ignored by political behavior research.

But application of these particular biobehavioral sciences alone is not enough. *Variations* in political behavior require explanation at least as urgently as basic uniformities. Ethology and evolutionary biology tell us little about the internal mechanisms underlying both the uniformities and the variations. Ethologists, for example, speak of "key stimuli" which can "trigger" "innate releasing mechanisms," which in turn effect complex patterns of pre-programmed behavior. And, to be sure, ethological research provides numerous well-documented generalizations about observed sequences of key stimuli and resulting behaviors. Unfortunately, such concepts as "innate releasing mechanisms" are identified with no identifiable physiological processes or anatomical structures within the organism. They are concepts no less insubstantial and ghostly than the social psychologists' attitudes, despite having a material-sounding name and an explicitly non-*mental* definition.

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[7] The fact that Aristotle considered the *polis* the "natural" form of human polity in a teleological sense, and that a great deal of his writing is devoted to normative implications of an ethical premise, should not obscure the fact that he recognized that other forms of polity besides the *polis* also did occur "naturally" in the empirical sense of the word. Aristotle was thus not only the first political scientist, but, indeed, the first *bio*political scientist.

IV

How other biobehavioral sciences come into play can be illustrated by further examination of the problems of maintaining the polity and obedience to political authorities. The problems have, of course, been dealt with by political science in various ways. There is a respectable body of literature on political obligation, but most of it concerns ethical problems of compliance or resistance. It sheds little light on the relationship of observed compliance behavior to proposed ethical guidelines. In political philosophy and in the general field of comparative politics there are some discussions of "legitimacy," both as a normative and as an empirical concept, but there is little systematic research on it.

Most empirical research literature, virtually all of it considered "behavioral," concerns "political support." This literature displays virtually all the conceptual and theoretical characteristics we have been describing. Resting on an almost exclusively "attitudinal" conception of human behavior, all the key variables, such as political trust, political alienation, political efficacy, and so on, are invariably conceived as broad mental images which can be verbalized. Data are collected by surveys and attitudes are measured by usually simple instruments and scales constructed from questions of the familiar survey type:

> (Do you agree or disagree), I don't think public officials care much what people like me think.
>
> Do you think that people in the government waste a lot of the money we pay in taxes, waste some of it, or don't waste very much of it?
>
> Do you think that quite a few of the people running the government are a little crooked, not very many are, or do you think hardly any of them are crooked at all?

And so on.

Not only the key variables defining political support, but also those investigated as dependent upon it are often attitudinal in character. "Attraction to radical rightist policies and programs," "negativism toward all things political," alleged to be the most common consequences of declining political support (Wright, 1976, pp. 77–87), are clearly such. Other dependent variables which appear at first glance to be direct behavioral reflections of these, such as abandonment of democratic procedures ("taking to the streets") or voting for rightist candidates, are almost always measured by self-reports about willingness to engage in or past engagement in such action, as are such variables as participating in riots or violent and aggressive behavior against police and others. The only behavioral correlate of the relevant attitudes indexed by reference to actual behavior instead of self-report is the decline in voting participation among those found to be politically alienated, but even that is just as commonly indexed by survey responses as by genuine behavioral data (Wright, 1976, pp. 251 ff.).

It seems clear that, unless there is some link between the basic political support attitudes in question and the genuine behavior variables of real interest to political science, research on the subject is likely to be uninteresting or trivial. The most extensive review of that research to date, finding no such link, seriously questions its value (Wright, 1976). The conclusion to be drawn with respect to further research is not that we have found out little because we have been examining the wrong attitudes, and that we should therefore look for others which will more reliably explain riots, rebellions, etc., but that we should abandon the notion that verbal self-reports about attitude items are isomorphic surrogates for supportive or non-supportive attitudes, and that all "overt behavior" is "caused" by such conscious attitudes, and should stop using verbal self-reports as dependent variables. To understand obedience to or rebellion against political authority requires taking a more realistic look at such behavior.

The Milgram experiments again offer a suggestive clue for doing this. In those experiments, researchers gathered not only question-and-answer data before, during, and after the experiment, but also some less systematic data about verbal and nonverbal behavior exhibited by subjects while complying with commands to "increase the shock level" they thought they were administering—exclamations, gasps, groans, intense perspiration, nervous or hysterical laughter, tensing, writhing, and so on. These data suggest as much about the internal state of the subject as verbal answers to questions. For verbal behavior *is* essentially *overt* behavior, and should be so studied. The fact that words can be used to express ideas does not exempt the physiology of speech mechanisms from responding to unconscious impulses. Whether a speaker is talking sense, nonsense, or turkey, physiological evidence of internal reactions, as measured by psychophysiological recording techniques, is often a better indicator of attitudes or emotional state than the overt meaning of words, and therefore, in an important sense, of the "meaning" of situations and stimuli. Viewing Milgram's subjects in this light makes it clear why it is necessary, as a leading psychophysiologist has recently urged,

> that we conceive of the human organism engaged in [the complex regulation of multiple psychophysiological processes] as being a symphony of biological organs that are orchestrated and conducted by the brain (Schwartz, 1978, p. xii).

Reactions to stimuli (such as Milgram's experimental commands), thoughts, feelings, verbalizations about the situation, memories of other situations, habitual responses to certain cues—all are embedded in a manifold of continually ongoing neural, hormonal, and other physiological activity. Human beings do not live and move simply and solely by motor responses to orders from the central nervous system, issued in response to cognitive and affective ideation. Simultaneous with any such activity there is always occurring integrally related activity of the autonomic nervous system and the glandular, hormonal, and other systems innervated by it. Much of the organism's emotional state and its physical readiness to do or not do certain acts (e.g., "fight or flight"), are the direct expression of the state of this neurophysiological system. Although the functioning of this system is not accessible to conscious introspective examination, save in its grossest aspects, the evidence of its working is observable in such psychophysiological activity as sweating of the palms, release of adrenalin into the blood stream, rise or fall in heart rate, increase or decrease in blood pressure, slower or more rapid respiration, and many others.

By observing psychophysiological responses in different situations and to different stimuli, and by observing other behavior of the individual at the same time, including verbal behavior in interview situations, psychophysiologists can tell us much more about the internal state of a person than answers to the usual survey-type questions can tell us. Research being done by Milton Lodge, Bernard Tursky, Joseph Tanenhaus and others at Stony Brook moves in this direction. In one study, for example, using classical conditioning techniques familiar in behavioral psychology, they conditioned half the subjects to react to stimuli connoting "white race" to them and half to react to stimuli connoting "black race." They then measured various physiological responses (heart rate, galvanic skin response, etc.) to ambiguous verbal and visual stimuli conceived of as political stimuli, judging from the subjects' response (or lack of it) the extent to which each perceived the stimulus as a racial one. Such studies promise to reveal more about the various political "meanings" of such symbols than the subjects' reports of their thoughts about them. For example, the overt (though internal) physiological response to the stimulus word "busing" by a subject who has been conditioned to respond to any stimulus perceived as having a racial connotation tells us more about probable overt motor behavior in a race riot than merely verbal answers to questions (Tursky et al., 1976; Wahlke and Lodge, 1972).

Stony Brook political scientists are also producing psychophysical tools which, on technical and statistical grounds, will give us far more reliable data about the verbal behavior of interview respondents and therefore a more sophisticated grasp of the possible mental-process correlates of verbal behavior. Put simply, the psychophysical scales produced by multimodal methods (i.e., methods using other modalities of response besides verbal, such as sound production, magnitude estimation, or line production) have the statistical properties not of simple categoric (nominal) or ordinal-level scales which is all that prevailing techniques permit, but of interval and ratio scales which will eventually permit cross-individual and cross-cultural comparisons of measurements made by them (Lodge et al., 1975, 1976).

Psychophysiological and psychophysical research is thus beginning to explain some variations in individual behavior and to enhance our understanding of the nature and function of verbal behavior, both in actual politics and in survey research. This work may seem far removed from the concerns of ethologists and evolutionary biologists; however, these two kinds of work and the different biobehavioral sciences on which they draw are not alternative approaches. They are equally valid, mutually supporting elements in a body of science which is producing a growing and interrelated body of general propositions about human behavior.

V

I will conclude not by attempting to summarize the already compressed discussion but by restating an important point: I have not been proclaiming new knowledge about political behavior but urging renewed attention to the task of acquiring some. I do not call for establishing a new discipline or converting political scientists to new faiths, but I do wish to call attention to a mounting set of facts of life established in a wide range of biobehavioral sciences, facts which have been not merely neglected in all but recklessly contradicted in some political behavior research.

Although I have been critical of political behavioralists, I would emphasize that I think the basic premises and objectives of political behavioralism are correct. Indeed, I think the only way to move political research beyond its current pre-behavioral state is to make it much more genuinely behavioral. This is not to say that political science must become "sociobiology," or that "biopolitics" is the embodiment of truth about political behavior. On the contrary, only genuine behavioral political scientists will be able eventually to answer the questions which are the real concern of political science.

But being a genuinely behavioral political scientist requires substantial mastery of at least the rudiments of a formidable array of biobehavioral disciplines. Moroever, in addition to being biobehaviorally literate, political scientists must also have the wisdom, insight and imagination of the best traditional political scientists plus the methodological skills of the best political behavioralists. Biobehavioral science may help political science move out of its pre-behavioral stage, but it does not make being a competent political scientist easier.

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Plus ça change . . .: The New CPS Election Study Panel*

PHILIP E. CONVERSE
GREGORY B. MARKUS
University of Michigan

Between 1956 and 1960, the first long-term panel study of the American electorate was carried out at the University of Michigan. Among other findings from this original panel were sharp contrasts between the high individual-level stability of party identification and more labile individual preferences on major political issues of the day. Since 1960, several changes in the nature of the American electoral response have caught the attention of scholars, including an erosion of party loyalties on one hand and an increasing crystallization of issue attitudes on the other. Completion of a new panel segment, 1972–76, makes it possible to review the original 1956–60 findings in the light of these intervening changes. We discovered that the contrasts in individual-level continuity of party and issue positions remain nearly identical to those estimated for 1956–60. The theoretical significance of these counter-intuitive results is discussed.

In some of our sister sciences, publication of frankly provisional findings, even in the most polished of journals, is not considered *déclassé*. This is particularly true where crucial new data bases have freshly come into being. Thus when the first rocks were returned from the moon, a haste spate of publications merely recounted, in the languages of various specialties, what the rocks were like in simple, first-cut terms.

To be sure, more delicate and incisive analyses were to be supplied in ensuing years. However, the function of such early publication for the collective process of inquiry should be transparent. Even the first crude observations as to the apparent age and structure of the rocks, or the presence of water or organic traces, sufficed to shoot down some broad-gauge astrophysical hunches as to what processes, on what time scale, had actually molded the moon's surface. And correspondingly, these primitive observations drew into sharper focus an appropriate research agenda among remaining tenable hypotheses for the months of more probing inquiry which lay ahead. Thus early publication can warn the community in advance which more detailed assays of the new data base are worth, and not worth, making.

Although the metaphor may be a little more dramatic than current political science can sustain, it is in some such spirit that this article is offered. Analysts of the sequence of national election studies generated with National Science Foundation help by the Center for Political Studies at the University of Michigan have been frustrated for some time at the lack of long-term panel linkages—the reinterviewing of the same respondents—in successive replications of the basic study design. The 1976 presidential election marked the completion of the first large-scale panel segment, stretching back to 1972, since the original four-year panel was completed in 1960. These new panel data constitute a fresh and important base which seems worth preliminary report.

Of course the study of electoral change has scarcely withered away in the interim for lack of full longitudinal studies. The use of less expensive independent samples at biennial elections has greatly enriched our understanding of the range of variation in American electoral processes since the first electorate panel in 1960, in part because the intervening period has been one of political change of sufficient magnitude to be plainly visible without the extra sensor of a genuine panel design. However, it is exactly this marked change that has put an increasing premium on the advent of a new round of panel data. Hence it will be helpful background if we review, with a brevity verging on oversimplification, some of the chief findings from the original panel, as well as revisions provoked by subsequent independent samples on into the early 1970s.

The Original 1956–60 Panel and Later Findings

Thanks to the delinquency of the current senior author, the major analyses of the panel-specific data from the 1956–58–60 reinterview sample were never reported in a self-contained monograph. However, some of the more surprising empirical facts to emerge from those analyses played an influential role in his essay "The Nature of Belief Systems in Mass Publics" (Converse, 1964). Stripped to its essentials, one brute fact which could only have arisen from long-term longitudinal data was the discovery that *at the individual level,* the stability of party identifications in the 1956–60 period vastly outstripped the stability of individual positions on even the most stable of the major political issues of the period.

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Such stability of party identification had been suspected for some time on the basis of the noteworthy inertia of marginal distributions for the variable in independent cross-section samples throughout the 1950s. In fact, that suspicion had been central in some of the major theses of *The American Voter* (1960). However, inertia of marginals, or minimal net change, is not at all incompatible with the possibility of high rates of *gross* change, or rapid individual-level "turnover." Indeed, while time series data on distributions of mass opinions on major political issues were somewhat more truncated because of the flux of salient issues from one election to the next, where repeated cross-section measurement did exist for political issues, the marginals were often as inert in the 1950s as those for party identification.

Therefore it was particularly striking to discover that beneath the net stability of party identification lay a very marked degree of gross, or individual-level stability; whereas the inertia or net stability of opinion distributions on major political issues concealed an equally surprising degree of individual-level turnover which appeared to be almost a Brownian motion. Let us be careful not to overstate the case. Party identification was *not perfectly* stable for individuals over the 1956–58–60 panel observations, in the sense that all individuals located themselves in the same one of seven possible categories in each ensuing measurement. Substantial numbers changed to an adjacent location on the continuum, and a very few appeared to move long distances (5–7 "slots") across the continuum. Similarly, it was not true that the individual-level data on issue positions showed no stability whatever. For one thing, there was significant variation from issue to issue, in patterns that seemed decodable; and in any event, even the issues with the greatest individual-level turnover showed some significant degree of continuity, as indexed by the correlation of each issue with itself over time departing reliably above zero.

Nonetheless, the contrasts in stability between party and issue positions remained absolutely stark, and we would find it hard to be impressed with any theory of the dynamics of these political attitudes which failed to encompass such contrasts. One implication seemed to be that party loyalties had a considerable primacy in the attitude systems involved. In the 1950s, at least, issue positions were but weakly aligned with partisanship, in the minds of the voters, in any event. Perceived linkages between parties and issues were, by and large, vague and contradictory as well. We opined at the time (*The American Voter,* pp. 179–83) that a major cause of the confusion might be that "actual" party differentiation on the major issues was at low ebb in the mid-fifties, a guess that later proved to have some merit. However, in the modest degree that partisanship and issue positions did covary, the contrasts in stability from the panel data suggested that it was likely that these issue preferences were more often brought into line with prior partisanship than the reverse. Once again, there is no need to exaggerate the case: surely issue positions could affect partisanship for occasional individuals at any time, and at some occasional junctures might do so for larger fractions of the population. But it seemed well-nigh impossible to square the panel contrasts in party and issue stability with a model in which the preponderant causal flow was, in any intermediate term, from issue position to partisanship, at least in the late 1950s.

Cross-Section Sample Findings from the 1960s. Although no full-blown panels of the national electorate were followed in the 1960s, there was ample sign of major trends in these matters by the middle years of the decade. While change was rapid on many fronts, some of the most impressive evidence involved the party-issue nexus.

Burnham (1968) noted that after a lengthy period in which the aggregate distribution of party identification had remained nearly constant, the first signal change began in the middle 1960s with a rising proportion of the electorate refusing to report identification with either party, and insisting on being classed as independents, thereby swelling the neutral middle of the party identification continuum. This apparent erosion in feelings of party loyalty among American voters, caught at an incipient stage by the first Burnham accounts, has since proceeded majestically in the same direction for close to a decade, and certainly must rank by now as change of massive magnitude. The trend has nicely fit Burnham's broader views as to the progressive "decomposition" of the traditional party system in the United States, and has become the subject of a torrent of literature analyzing details of the trend, or speculating as to its implications for partisan realignment.

Where issue positions of the public are concerned, Nie (with Anderson, 1974) has advanced some provocative analyses suggesting that soon after 1960, voter policy preferences began to take on a much firmer "muscle tone" than was characteristic of data from the 1950s. This change is largely indexed by a marked increase in the cross-sectional correlations among issue positions in the 1960s, as opposed to their feeble level of the preceding decade. The reality of the issue trends charted by Nie has been subject to somewhat more controversy than attends the brute empirical facts of erosion in party loyalties, since some (Bishop et al., 1978; Sullivan et al., 1978) have suggested that the changes may be more an artifact of shifts in question format in the Michigan series than true secular change. However this matter may be resolved, the Nie thesis has strong face validity at least, since increased public attention to policy cleavages is almost another way of describing the onset of political turbulence of the type characterizing American politics for the past dozen years.

Moreover, the Nie thesis receives supplementary support from changes observed in linkages between partisanship, issue positions, and voting decisions. For example, Pomper (1972) pointed out that voter perceptions of party differences on policy issues showed more clarity by 1964 than had been true in the first panel period of the late 1950s. The citizen was more likely to see differences between the parties on policy matters, and the alignment between partisanship and issue preferences had heightened as well. In the most exhaustive recapitulation of these trends, Nie, Verba and Petrocik (1976) have shown that the election-by-election correlations between party identification and the vote, as compared with issue-vote correlations, changed after the early 1960s: the apparent weight of partisanship in the voting decision had undergone significant decline, while the weight of policy preferences showed a marked advance.[1]

Again the magnitude of these trends, as well as their durability into the 1970s, remain subject to scholarly dispute (see, for example, Margolis [1977] on Pomper). Similarly, the longer-range import of the differences before and after the early 1960s is subject to basic disagreement. Some see these changes as secular and permanent or even accelerating into the future. Others see one or the other political period as the American norm, interrupted by an era of aberration. Still others see the two periods as local minima and maxima, with a putative normal lying somewhere between.

Nevertheless, some of the component changes are beyond dispute as empirical facts, and taken as a *gestalt*, they have a great deal of intuitive appeal. The parts fit together admirably, and correspond nicely with impressions most of us who lived through both periods would have harbored in any event.

It is against this backdrop that the advent of the new 1972–74–76 panel version of the Michigan election series is particularly welcome. After all, the trends catalogued since 1960 have all been developed without the longitudinal tracing of the same individuals over time. This is not to say the trends are spurious: the movements of marginal distributions and drifts in correlations coefficients estimated within each slice of time would certainly have been duplicated (apart from sampling error and variation due to population replacement) if panel studies had been mounted throughout the era since 1960.

However, one precious class of information—the continuity or individual-level stability of the key variables—cannot be squeezed from the independent-replication design. In fact, it cannot even be deduced from other attendant slice-of-time facts with much reliability, as we shall shortly see. And given the intervening work on electoral change, some of the first-cut expectations as to how the new panel data might look are extremely straightforward. Apparently party is down, and issues are up. The main question to be asked of the new panel data would merely seem to be "by how much?"

We shall reveal the general answer rapidly, for its explication can easily occupy us for the rest of this article. Where party and issues are concerned, and *with respect to those facets of change which a panel design is uniquely equipped to illuminate*, our first-cut explorations show that *the 1972–76 panel data are very nearly a carbon copy of those from the 1956–60 panel!* That is, the individual stability of party identification, relative to that for policy preferences, is just about as great for the latter period as for the former.

We shall spend most of the rest of this article trying to clarify just what those initial results do and do not mean. One thing they do not mean is that no change has occurred over this period. A severe handicap in approaching problems of change consists of the first simplistic assumption that change with respect to a given variable is something monolithic: either the variable changes or it doesn't, in which case we might not want to call it a variable. Such a statement can have merit if we specify it more closely, as we might by referring exclusively to the mean or central tendency of the variable. Then it is undoubtedly true that either the mean has changed or it has not. But more generally speaking, change is multifaceted, even if we are addressing the same variable in its behavior over time.

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[1] Presidential voting in the 1976 election seems to be the first major reversal of this trend.

These multiple facets of change, even with respect to a single variable, are recognized in more formal treatments of process. Although the distinctions are looser than one might like, processes are described as "strictly stationary," only "weakly stationary," or "not stationary at all," according to whether all, some or none of the moments of the univariate distribution are constant. Thus each moment is, unto itself, a facet of change. As we shall see momentarily, there are still further facets of change with respect to a single variable, in addition to the moments, and it is one of these which shows unexpected constancy or stationarity.

It is exactly because impressive changes are known to have occurred in the party and issue variables that the panel discovery of any points of stationarity is particularly exciting. In a period in which multiple facets of change are occurring, the localization of some points of stationarity are theoretically pregnant in diagnosing the nature of the processes underlying the total manifold of observations. To paraphrase Archimedes, they give us "a place to stand" from which other facets in flux can be evaluated.

The Case of Party Identification

We can profitably turn to the case of party identification over the comparative 1956–60 and 1972–76 time segments for concrete examples of the varied facets of change. All too frequently, contradictions appear to arise in the treatments of change carried out on the same variables by different scholars, simply because different aspects of change are being addressed, and for quite different purposes. This potential for confusion is particularly virulent as we approach the panel feature of these change data, so careful description is important.

The most obvious facet of change in a single variable based on aggregated observations over time is the displacement of the mean of the distribution from one reading to the next. Several authors dealing with the first panel have examined changes in the central tendency of the party identification variable in the 1956–60 period. There are various ways such change can be indexed in addition to the obvious integer scoring of the conventional seven-point scale and taking an actual mean. One can deal, for example, with the percentage Democratic (or Republican) of the total sample, or of the more restricted set of those who identify with one party or another. If, for example, we examine the marginal distributions of party identification in the panel years in terms of percentage Democratic (strong or weak) of all *identifiers*, we find values of about 60 percent for 1956, 64 percent for 1958, and 61 percent for 1960. For 1972 the value is 63 percent, for 1974, 65 percent, and for 1976, 63 percent. These are forms of change.

Such statements of change can be conceived as referring to shifts in the mean or central tendency of the party identification distribution, as one facet of change. The shifts are customarily small. If we stick to the percentage Democratic of all identifiers, the median biennial displacement over the whole election study series runs below 2 percent. On the other hand, unless they can be thought of as sampling error, these differences are in themselves evidence that party identification is not perfectly stable down to the last voter. Although they are typically small enough to lie within the tolerances of plausible sampling error, it has long been clear however that these shifts are quite systematic, moving nicely in tune with national short-term tides favoring one party or the other. For example, the three biennial shifts since 1952 which exceed 3 percent are all associated with landslide presidential elections. More impressively still, first differences in these percentages Democratic among identifiers from one biennial election to another show a resounding correlation of .75 with first differences in the Democratic fraction of House seats after the corresponding elections since 1952. Clearly, although these shifts are slight, we are not dealing with mere sampling error in this facet of party identification change. Indeed, Brody (1977) in working with the 1956–58 and 1972–74 segments of the panel studies, focuses a great deal of his attention on these shifts, showing within the panels themselves that shifting has political meaning in terms of other attitudinal dimensions.

How important, theoretically and practically speaking, depends entirely on the more specific class of questions one wishes to ask about change. Certainly for any problems of short-term changes in political tides, they are a crucial indicator, although it must be recognized that a disproportionate share of these swings come from people who do not vote in any event.[2]

On the other hand, if one's research interests concerning change are longer-run in their perspective, then these small shifts are not of gripping interest. This is not simply because they are small, but also because their nature as short-term oscillations around a mean value has historically been quite clear. If the proportion Democratic shifts by 4 percent between two biennial elections, as it did between 1956 and 1958, this is not likely to be an intriguing harbinger of fundamental partisan change, because the odds are very high that the shift will rapidly be reversed. It is as though we were watching reeds growing in a shallow pond. Before an east wind, they tilt to the west. Before a west wind they tilt to the east. When the wind is still, they stand in a middling position. From some points of view the motion is of interest, since it tells us which way the wind is blowing at the moment. From some other points of view it is not of great interest, since we know the reeds are not moving in any absolute sense, but are firmly anchored in a fixed position by their roots.

Speaking more technically, the mean of the party identification distribution around which these small oscillations occur has been almost stationary for the past 25 years. We say "almost" because there appears to have been a glacially slow drift of the mean in a Democratic direction over this period, probably due to population replacement. Even in 25 years, the total ground covered is very small. This secular drift is small by comparison with the amplitude of the oscillations about that mean, even though those oscillations are in turn very small by comparison with the swings of the national popular vote.

It is important to recognize these oscillations for what they are in the panel data, to avoid misinterpretations of the changes they generate. Thus, for example, Brody (1977) discusses the counter-intuitive finding that in *both* the 1956–58 and the 1972–74 segments of the panel studies, Republican identifiers are less "directionally stable" (i.e., more likely to move out of the Republican camp into at least independence if not a Democratic identification) than Democrats, despite the fact that in most other terms Republicans have seemed more "anchored" in their political behavior. This perplexing effect almost certainly arises because both the 1956–58 and the 1972–74 segments bracket major shifts in short-term partisan fortunes from the Republicans to Democrats. Thus even relatively labile respondents who claimed to be Democrats when current winds were favoring the Republicans (1956 and 1972) had little reason to stop being Democrats two years later when the winds had become more supportive. Presumably the reverse partisan contrasts would occur in temporal comparisons where the wind changes were simply reversed.

While the mean of a distribution provides one facet of potential change in a variable, it scarcely exhausts the subject of change where that variable is concerned. Aside from practical problems like ceilings and floors on measuring instruments, the variance of the distribution can move independently of the mean. It can remain stationary while the mean shifts dramatically; or it can shrink or expand dramatically while the mean is stationary. In fact, as we have seen, the most striking change in party identification since 1965 is a shrinkage of the variance by more than 10 percent, as more and more voters cluster toward the independent middle of the distribution. This is again true change in party identification, and has appropriately been treated in the recent literature as having major theoretical interest. Yet this change has occurred rapidly while the mean of the distribution has been essentially constant.

There is no need to do a panel study to monitor change in either the mean or the variance of a distribution. Successive independent cross-section samples can perform this task equally well. The information a panel study uniquely supplies involves the continuity in positioning of the same individuals over time. One index of such continuity is the proportion of the panel on or near the main diagonal of the cross-tabulation of the same variable between waves. While such expressions are commonly used, they are weak summarizations not only because of the arbitrary nature of the category width that constitutes the "stable" diagonal, but also because the resulting proportions are strongly influenced by the number of categories into which the variable is partitioned, so that comparisons across variables of different category "length" become nonsensical. An obvious improvement is to deal with a summary expression of correlation or regression.

We have approached the new panel data on party identification only after a consideration of changes in means and variances for a very simple reason: although novices are constantly confused by the fact, continuity correlations formed on the same variable in two panel waves are as independent of temporal changes in the mean and variance of the distribution as the latter are independent of one another. That is to say, a continuity correlation can range from zero to perfection whether both mean and variance are constant, or one or both shift dramatically between the two time points. This is true by the very construction of the product-moment coefficient, which equates or normalizes the two means and variances, thereby partialling out whatever real change may have occurred with respect to those facets of the variable.[3]

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[2] For a more extended discussion of the significance of these oscillations, see Converse (1976), pp. 126–30.

Since continuity correlations can vary independently of changes in means and variances, they obviously tap still another facet of change. It is important to be clear just what facet that is. Obviously, it has to do with stability of individual-level positioning. What is important to remember is that it taps the stability of *relative* individual positions across a population, not absolute ones.

While it is thereby technically quite possible that individual-level continuity correlations for party identification have remained at their high levels of the late 1950s despite the shrinking variance of the distribution, it would not intuitively seem very likely. After all, the descriptors commonly applied to the surge in independence talk of the "erosion" of these identifications, and the "destabilization of partisanship." While such terms are entirely proper, they would certainly seem to imply a major unraveling of individual-level continuities in party preference, even in the stability of relative locations. Moreover, at a less intuitive and more technical level, if error variances remain constant while true variances of a variable decline, then the magnitude of correlations involving the variable will be attenuated.[4] Thus the natural shrinkage of observed variance of party identification might be expected on these grounds alone to dilute the continuity correlations correspondingly.

Some years back we suggested that this might not turn out empirically to be the case with party identification (Converse, 1975), since the only scrap of a national panel conducted during the 1960s—a couple hundred stray cases reinterviewed between 1964 and 1966—showed continuity correlations which were virtually identical to those estimated from the 1956–60 panel, despite the fact that this panel lapped over into the period in which the variance of identification reports had begun to shrink palpably. Therefore we are not entirely surprised to discover that the continuity correlations calculated for party identification from the new 1972–76 panel are just about as high as they were in 1956–60. In absolute terms they are a hair smaller, but the difference is too slight for statistical significance. Probably the drop is in some degree real, as one would expect from the truncation of variance in the later period. However, the stunning fact is that the decline is simply vestigial: if one could say from the 1956–60 panel that individual-level continuity correlations for party identification with a two-year time lapse ran in the low .80s, one can make exactly the same statement for the 1972–76 panel.

For reasons that should be apparent, the magnitudes of continuity coefficients are specific to the size of the time interval elapsing between measurements. In processes of progressive (i.e., not cyclical) change, they can be expected to decline as the interval becomes more extended. Indeed, this property emerges clearly in the party identification data. The four-year correlations are less than those calculated for a two-year interval; and the latter are smaller than correlations formed for measurements separated by only a few weeks in time, as is the case when the party identification item is applied to the same sample in both the pre-election and post-election questionnaires. Therefore continuity correlations in general only have meaning as they are tagged with some specific time interval. If we stick with the two-year correlations in order to encompass comfortably the data from the two-wave, 1964–66 mini-panel, then we find the following best estimates for two-year continuity correlations:[5]

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[3] Obviously, one can restore some of this information in relatively compact form by shifting from correlation coefficients to raw or nonstandardized regression coefficients plus intercept terms. For some purposes this is exactly what one should do, although such statements operate in a metric-bound currency which can impede communication for those unfamiliar with the variable. For simplicity we shall deal here with continuity correlations.

[4] Note, however, that this expectation is contingent on an error variance which is gaining in size relative to the true variance. In the more general case, the continuity correlation is in theory independent of the variances of the component distributions.

[5] All correlations are calculated on the basis of party identification as a seven-point, integer-scored scale. Here as at some other points in this article,

The 1956–60 period = .835
The 1964–66 mini-panel = .836
The 1972–76 period = .813

There are several ways of processing these raw continuity coefficients into more refined components. One of the easiest partitions, dependent on having at least three waves of measurement, is to break the raw coefficients into (1) a reliability component, reflecting the complement of the proportion of error variance; and (2) a "true" stability coefficient, reflecting the degree to which the measurement at two points in time would be correlated if there were not intrusion of unreliability. Such procedures have been described by Heise (1969), and refined a further step by Wiley and Wiley (1970). These manipulations rest, as always, on a series of assumptions about the character of the data and the attendant error structures. One of the less pleasing sets of assumptions—that the measurement reliability (Heise) or the absolute error variances (Wiley and Wiley) are constant from wave to wave in a given panel sequence—can be relaxed in the case of the 1956–60 panel, where instead of three waves of measurement of party identification we can profit from five.[6] While it is regrettable that party identification was measured only three times in the new panel (1972 pre-, 1974, and 1976 pre-), there is sufficiently striking consistency between the two panels to enable us to use the additional information afforded by the first panel to reassure ourselves concerning various assumptions which must be made for the later batch of panel data. Also, of course, we can proceed no further with the mere two waves offered by the 1964–66 mini-panel, although the data presented above also give some assurance that we are dealing with process parameters which have a considerable stability in the intermediate term.

There are other assumptions embedded in calculations of the Heise or Wiley and Wiley type that are not entirely well met by the data. Thus, for example, there is reason to believe that both the measurement reliability and the true stability of the party identification item is less than nicely homogeneous over the population. In later, more complicated assays of these data, we shall take such characteristics into fuller account. Nonetheless, as first-cut approximations, it is worth seeing what can provisionally be estimated concerning the relative proportions of change due to unreliability, as opposed to "true change," in these longitudinal measurements.

Where they can be compared most fully (as in the five-wave panel), calculations of the Heise and Wiley and Wiley type give virtually identical estimates with these data. Both calculations agree that the main source of the slight decline in overall continuity between the two large panels is a slight increase in unreliability, exactly as one would expect if the main change in the data were a shrinkage in the true variance.[7] In both calculations, the decline in reliability is roughly from (a central tendency of about) .88 in the first panel to about .84 in the second.[8] Once this slight decline in reliability is set aside, as it is when we calculate the true stability coefficients, we find comparisons of the following sort as our best estimates for the two-year interval:

	1956–58–60	**1972–74–76**
Two-year stability (Heise style)	.951	.972
Two-year stability (Wiley stule)	.958	.972

The homogeneity of these estimates, both between calculation methods and more espe-

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precise numbers of cases are difficult to provide, since the estimates are more often than not "synthetic," resting on multiple bases. Thus, for example, the value of .835 is based on an averaging of four different estimates available in the 1956–60 panel for the correlation after about a two-year interval. While all of these values are very similar in appearance, each is based on a rather different number of cases.

[6] In addition to the full-blown fourth wave present on ICPSR tapes due to a full-sample application of the party identification item in the post-election as well as the pre-election study for 1960, a stray set of 165 cases were re-asked the party identification question in the 1956 post-election study as well. Estimates from this mini-sample are of course less stable than their counterparts in the four other waves. However, where comparisons are possible, they are so gracefully consistent with what can be learned from the pre- and post-1960 waves that we do not have much hesitation in using them to yield the additional degrees of freedom provided by a fifth wave. Naturally, in all relevant calculations, we take account of the fact that the points of observation are very unequally spaced in real time.

[7] Actually, our detailed calculations show not only no shrinkage in the error variance, but if anything a slight absolute increase. Thus the decrease in reliability (or the proportion of true variance to total variance) has a double source.

[8] Throughout this article we are providing mere "bottom-line" estimates that conceal more detailed consideration of angularities in the data. These will be dealt with more explicitly in subsequent technical

cially across two periods in which the nature of party identification has seemed so different, when other facets of change in the variable are considered, is very striking. There are a variety of ways of expressing the implications of these findings. We shall postpone some of the broader theoretical implications for our conclusions. For now, we might briefly consider what the above data imply for the long-term "staying power" of party identifications at the individual level.

If we merely knew that the raw two-year continuity correlation for party identification was about .82 (as a period-free average), then we might be tempted to conclude that over a period of 16 (2^4) years, we could only expect a continuity of .45 (or $.82^4$), a value which would not be entirely impressive. However, if we trust the above estimates as to the proportion of observed change which is mere unreliability of measurement, then we would expect the 16-year *observed* (i.e., error-attenuated) stability coefficient to look something like .73 (or .86 [$.96^4$], where the .86 is the period-free reliability estimate) instead of .45. This is a vastly different picture of the long-term individual-level stability of the variable, especially as it is appropriate to square these two values, yielding a contrast in shared temporal covariance of .53 instead of .26. And we feel considerably less cavalier about cycling hypothetically over longer periods of time in this way, once we have at least some shred of assurance, which the above data give, that the true stability of the party identification measure is remarkably close to stationary across two political periods in which, in other senses, it has been subject to major change.

However, even this assumption is subject to challenge. After all, the decline in reported strength of partisanship has proceeded secularly from 1965 to 1975. While our panel observations in 1964–66 and 1972–76 bracket the beginning and end of this fascinating period, they leave uncovered a crucial segment of time from 1966 to 1972. Perhaps it was in this six-year interval that continuity correlations on party identification might have registered a distinct sag.

Here again we can import a crucial datum which suggests that the stationarity of these continuity correlations has been complete over the past two decades. In 1973 Jennings and Niemi (1977) reinterviewed the parents of their 1965 graduating high school students, who had originally been interviewed in 1965. In principle this is a 1965–73 panel, covering exactly the years that we are missing in our national samples. The difficulty is that these parents cannot be seen as a proper sample of the adult electorate, even though they may be a perfectly honest national sample of a more restricted population.

What the Jennings-Niemi respondents have in common is having been parents of high school seniors in 1965. This means that they are a very homogeneous age cohort, relative to the electorate as a whole. The group lacks newer and younger voters completely and, given their association with children not dropping out of high school, they should show higher than normal social status. Now it can be calculated that if such respondents were really representative of the full electorate, and if our panel continuity coefficients were truly stationary in the long run, then over an eight-year span these respondents should show a continuity correlation for party identification of .76. However, the age and class bias of the Jennings-Niemi respondents would lead one to expect a slightly higher figure. It turns out that the observed figure for the party identifications of the Jennings-Niemi parents over the 1965–1973 interval is .79. This value is either right on target or, if anything, a shade high: certainly an hypothesis which maintains that continuity sagged between our panel points of 1966 and 1972 finds no comfort whatever in these data. All of the available evidence, then, suggests that there has been no change in the individual-level continuity of party identification over the past 20 years, despite an incontestable change in the likelihood of reported partisanship.

The Continuity of Policy Preferences in the '50s and the '70s

A comparative examination of the individual-level stability of preferences on political issues poses somewhat different analysis problems, chiefly because while the two major parties have long been constants of the political scene, the objects of issue orientation are in perpetual evolution. The issue positions which seemed central to political controversy in the early 1970s were somewhat different from those central in the late 1950s. Moreover, the

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articles. One such angularity being momentarily ignored in this statement, although dealt with in our calculations, is the presence of some systematic progression of the unreliability term across waves of the same panel.

election studies have been for some time measuring issue positions in a different format than was used in the late 1950s, and there are reasons to expect that these pure methods changes should affect some of the process parameters which interest us (Bishop et al. and Sullivan et al., 1978).

Hence strict comparisons of issue continuities are impossible. Nevertheless, there are some matches between issue items from the panel studies of the two periods which are tighter than others, so that some rough comparisons are possible. Moreover, the continuity correlations for issue positions in the original panel fell within a range which was vastly different from the values for party identification, and therefore it is of interest to see how even the newer issues are behaving in these terms in the more recent panel. We shall begin with a few of the closer matches and then summarize the general trends over all of the new panel issues as a set.

One issue item in the later panel was worded identically to the original 1956–58–60 study. This is a foreign policy question, in agree-disagree form, intended to tap isolationism:

> "This country would be better off if we just stayed home and did not concern ourselves with problems in other parts of the world."

Despite the identical wording of questions, there were minor variations in format. Most notably, the item responses were handled dichotomously in the new panel, whereas they had been spread out into four main points ("strong agree" to "strong disagree") in all waves of the original panel. Also in the last two waves of the first panel, a "filter" was added to assess whether respondents felt they had an opinion on the matter. Both of these format differences would seem likely, by conventional wisdom at least, to dull the correlations surrounding the more recent application of the isolationism item, other things equal.

Unfortunately, this foreign policy item was asked only in 1972 and 1976 in the second panel, so we can only make direct comparisons with the four-year (1956–60) continuity correlation from the first panel. Since the same limitation applies to the only other plausible foreign policy item match, we shall take that item in the same breath, even though item wording is much less parallel. Both panels had an item on foreign aid, although with the following variation in wordings:

> (1956–60) "The United States should give economic help to the poorer countries of the world even if those countries can't pay for it."
>
> (1972–76) "The United States should give help to foreign countries even if they don't stand for the same things we do."

The same differences in response categories pertain for these foreign aid items as for the isolationism item.

The four-year continuity correlations for these foreign policy items are as follows:

	1956–60		1972–76	
	r	(N)	r	(N)
U.S. Stay Home	.347	(1086)	.309	(1113)
Foreign Aid	.292	(1009)	.264	(1039)

The similarities between the two panels are quite impressive. And if there is any merit in our a priori expectation that format differences would tend to attenuate the more recent panel results by some small amount, it would be hard to argue that these continuity estimates are any less stationary than those for party identification, although of course they fall at a totally different level of correlation.

In the original panel, foreign policy preferences tended to be among those issue items of lesser individual-level stability. We shall see that the case is exactly the same in the new panel. Domestic issues in the old panel fell into two substantive domains: social welfare policy and civil rights policy. Both domains are represented with three-wave issue items in the new panel, although the item format is always different, and the wording shows greater or lesser variation.

The nearest thing to a match among the social welfare items involves questions about government guarantees of employment. The old agree-disagree item has been changed to a forced-choice format, with two contrasting substantive alternatives. Moreover, in this case—in contrast to the foreign policy items—response alternatives for the new panel are *more* numerous than for the old, with a 7-point rating scale being used to replace the four-category agree-disagree form. Thus, by the same reasoning as used before, we might expect slightly higher correlations with the new panel than with the old. Despite such differences in form, the intent of the two items is surely identical:

> (1956–60) " 'The government in Washington ought to see to it that everybody who wants to work can find a job.' Now would you have an opinion on this or not? (IF YES) Do you think the government should do this?"
>
> (1972–76) "Some people feel that the government in Washington should see to it that every person has a job and a good standard of living. Suppose that these people are at one end of this

> scale—at point number 1. Others think the government should just let each person get ahead on his own. Suppose these people are at the other end—at point number 7. And, of course, some other people have opinions somewhere in between. Where would you place yourself on this scale, or haven't you thought much about this?"

For these items, asked in all three major waves of both panels, the average two-year continuity correlation in each study is:

	1956–60	1972–76
Government Job Guarantee	.457	.493

Again, we may well be struck by the similarity in values between the two periods, especially by comparison with the much more stable party identification items, or the visibly less stable foreign policy questions. Or if we want to pay attention to the small differences in absolute values between the two estimates, the value for the new panel is in fact slightly higher, as format changes would have led us to believe.

In civil rights, an item on school desegregation occurs in both panels, although it is another instance in which the question was only asked twice (1972 and 1976) in the second panel. The latter item is a forced-choice version of the earlier form, with those respondents claiming no opinion being filtered off in both instances.

> (1956–60) "The government in Washington should stay out of the question whether white and colored children go to the same school."
>
> (1972–76) "Do you think the government in Washington should see to it that white and black children go to the same schools, or stay out of this area as it is not its business."

In general we would expect the forced-choice version to produce slightly more discriminating measurement and hence higher correlations; yet the item in the second panel was only scored as a dichotomy, instead of the four-point agree-disagree scale used originally. Therefore there are no very clear expectations as to pure methods differences in the continuity correlations. The actual four-year correlations are, however, very similar:

	1956–60		1972–76	
	r	(N)	r	(N)
School Desegregation	.397	(1059)	.410	(714)

By this time it will not be surprising if the reader is beginning to find the parallels between all of these panel results, despite a decade and a half of intervening political change, as eerie as these parallels have seemed to us.

We have by now exhausted all of the issues for which very close matches in wording and intent, if not in format or in letter, are available.[9] While the similarities in individual-level stability for these well-matched items are striking, we should not conclude that the issue materials from the new panel more generally look just like those from the earlier data set. This is true chiefly because there are new issues present in the 1972–76 panel which are not inheritances from the past; and these items sometimes have a different cast in continuity terms.

A good example is provided by the civil rights issue concerning school busing to promote racial integration. This issue was not even in the air at the time of the earlier panel, so that its absence from those interviews was not at all a matter of editorial oversight, but rather a token of a truly different environment of policy debate. The issue is also of particular interest because casual observation would suggest that this issue, probably more than any other in the civil rights area or, for that matter, the total arena of policy debate, has had an inflammatory or polarizing quality that has bitten deep into even relatively inattentive segments of the public. In other words, if as we have earlier suggested, it is true that the general levels of these continuity correlations reflect the degree to which items tap into more or less thoroughly crystallized attitude structures (where the limiting case of no crystallization whatever is the "non-attitude"), we would expect rather substantial continuity correlations for the busing items. This expectation is sustained. The best estimate of the two-year continuity correlation is .575, a value still falling very well below the corresponding value for party identification, but nonetheless significantly higher than that registered for any issue items in the whole 1956–60 panel.

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[9] There is one other pairing, also in civil rights, where a somewhat rougher match might be made. In the first panel, a question inspired by the FEPC legislation of the time asked about the appropriateness of the government attempting to ensure that Negroes get "fair treatment in jobs and housing." In the second panel, respondents were asked whether the government should make "every possible effort to improve the social and economic position of blacks and other minority groups." These are divergent questions, both in substance and in group of reference, although there is some obvious kinship between them. The first item was in four-point agree-disagree form; the second was in forced-choice, seven-point scale form. The average two-year continuity correlations in the first instance was .468; in the second, a somewhat higher .535.

There are at least two other issues new to the 1972–76 panel which show even higher continuity values. These items are not only new: together they seem to stand for a coherent *class* of issue which was not even of much salience in the 1950s. One is a 7-point scale where the extremes involve the legalization of marijuana at one end, and the setting of higher penalties for its use at the other. The other is an item on abortion, in which the respondent is asked to choose between four levels of lenience in availability, essentially bounded by "under no circumstance" at one end and "at any need" on the other. Neither of these items was asked in the middle wave in 1974, so that only estimates of the four-year continuity correlations are available. As we have pointed out, coefficients over this time lapse tend to run lower than their two-year counterparts. Thus, for example, if the school busing issue runs about .574 for a two-year lapse, it stands at .535 for a four-year lapse. The only two issues which exceed it are marijuana and abortion, with *four*-year coefficients of .640 and .617, respectively.

These are high values, and they seem clearly associated with a class of moral issues of the kind brought to salience by the counterculture confrontations of the preceding decade. Indeed, the only other specific issue item with a four-year continuity correlation over .50, in addition to marijuana, abortion and busing, is an item on whether women should have an equal role with men in business and government, or should keep their place in the home (.519). Again, this is an item which pits the cutting edge of new mores against an array of traditional values.

For those who have worked in other political cultures, it is not entirely surprising that such moral issues should have a deeper resonance among those not normally attentive to much political controversy than is true for policy debates of most other kinds. Perhaps these uncommon magnitudes represent something other than a sharper crystallization of attitudes across a larger fraction of the electorate than is true of other issues, but for the time being such would seem to be the most plausible diagnosis.

There is one other item, not referring to a specific policy issue or for that matter any "new" issue at all, but relevant to a large range of issues, which also shows a four-year continuity correlation over .50, although the high value occurs under a special configuration of circumstances important to make clear. This item is the ideological item involving self-placement on a 7-point liberal-conservative scale, with a four-year continuity correlation of .564. The special circumstances arise because the correlation is being figured on a much more limited fraction of the population than is true of any of the other specific issues. This high attrition occurs because such a continuity correlation can only be computed for those respondents who choose some content response at both years involved in the correlation. On items like the abortion question, almost all respondents choose a content alternative, so that the continuity correlation refers to a quasi-totality of the electorate. On the liberal-conservative item, however, over one-third of the population must be set aside at the outset as missing data, either because they say they do not know their position or because they accept a "no opinion" filter.

It may seem surprising that only as few as 35 percent or so fail to give content locations on two successive administrations of this ideological scale, and hence do not figure in the continuity correlations, when for many years items asking people what the labels "liberal" and "conservative" mean have shown some 40 percent or so who do not know. The implication is either that some people who would not profess to know what political difference is intended are nonetheless choosing a location on the continuum, or that the 40 percent value has changed, although it had not changed even after we were deep into the turmoil of the 1960s. Unfortunately, the item tapping the meaning of the terms was never asked in any of the five waves of the new panel. However, work in Western Europe has made it clear that people who locate themselves on an analogous left-right scale despite confession that they do not understand what the terms mean politically, tend in large numbers to place themselves at the exact midpoint of the continuum offered (e.g., Converse and Pierce, 1970). This is an easy solution when one is unsure of the significance of the extremities. And indeed, the midpoint of the United States scale is very well populated also. In fact, in the turnover cross-tabulations of the liberal-conservative scale, the cell which marks the exact center on both administrations tends to be two to three times as populous as any other cell in the matrix.

Hence the special circumstances which appear to sustain the high liberal-conservative continuity correlation seem to be that better than one-third of the population is removed at the outset; and among the remaining two-thirds, substantial numbers express their uncertainty by persistently locating themselves at dead center, thereby not adding to the continuity correlation, but not detracting much from it either. It is likely that for those with truly crystallized positions on the liberal-conservative continuum, the individual-level stability of position is very high, even as high or higher than party identification. But this fraction of the population is likely to be quite limited.

In our discussion we have introduced a number of issues or meta-issues which were not present in the original panel, but are new entrants in the 1972–76 period. All of these items discussed show higher levels of continuity than the most stable issues in the original panel. This might lead to a conclusion that levels of continuity in issue positions are up quite notably, if not because of heightened continuity from issues that can be specifically matched across the panels, at least because issues newly arisen in the real world are of a kind that tap more sharply crystallized opinion.

One can defend a very modest version of this argument if one limits attention to the two or three moral issues. However, a number of other "new issues" in the panel have not been discussed, and these generally show levels of continuity that are utterly redolent of issues in the 1956–60 period. Thus, for example, two items which have overtones of the "law and order" themes of the past decade—one dealing with repression of urban unrest, another dealing with protection of the rights of accused persons as opposed to stopping crime at all costs—show two-year continuity coefficients of about .36 and .45 respectively, values which are very middling relative to the range "normal" for domestic issues late in the 1950s. An item addressed to the possibility of a more or less progressive taxation rate shows a four-year value of about .31, and the figure is only .28 for an item asking whether the federal government has become too powerful. The latter figures are *lower* than any parallel figures to be found for domestic issues in the original panel.

To summarize, then, we have found that where specific issues can be directly matched, continuity values seem amazingly stationary across the two panels. One type of issue not salient in political debate in the late 1950s—moral or counterculture issues—do show signally higher continuity values than any other specific issues in either panel. Once beyond these moral issues, however, even new issues do not in general show any higher levels of continuity than the old.

A great deal of work of a more probing sort can be done with these issue materials. For example, more intensive investigations of the apparent substantial error structures in these responses are possible, particularly as some few of the new panel issues have been applied on four waves instead of merely three. Such empirical wedges are important to arrive at a proper diagnosis as to what these error structures really mean. Achen (1975) has concluded that the low levels of continuity marking issue responses occur simply because the items are unreliable, probably because of poor wording. It was the amazingly low "test-retest" reliability of the 1956–58–60 panel issues that touched off our earlier investigations as to why this degree of unreliability occurred, and led to our conclusion that the low apparent reliabilities were not simply a function of the items themselves in a shoddy-wording sense, but rather were a joint function of the substance of the items and the variable degree of attitude crystallization which more and less attentive respondents brought to the particular substance (Converse, 1964, 1970).

We still feel our interpretation is preferable to Achen's revision. Among other things, Achen's explanation does not give us much help in understanding why high political elites show higher continuity coefficients and hence apparently higher reliabilities than does the mass public, even when asked to respond to the very same simplistic issue items, although this difference is the most direct corollary of our original interpretation. Nor does the Achen interpretation prepare us to expect that the same team of question-writers who manage to word questions quite effectively regarding personal morals cannot find comparably effective wording when asking about foreign policy debates. Again, we need only imagine that disagreements about personal morals grip more people more vitally than do debates over foreign affairs in order to fold the new findings nicely into our original interpretive structure.

The important point is that the new panel data base offers a challenging field for investigating some of these differences more thoroughly.[10] For the moment, however, it seems fair to conclude that the individual-level relative stability of issue positions in the American public has changed only slightly between the two panels, and that no significant change whatever can be discerned where the same issue substance can be matched across the two eras.

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[10] We postpone consideration of the detailed partitioning of the issue items into reliability and stability components until a subsequent technical paper, since a proper treatment requires taking account of differing response variances in different population segments. These complications are too elaborate for space available here. However, it may be of interest to note that a simple partitioning of these issue items, ignoring the complications, suggests that the unreliability component of responses greatly outruns a true change component, just as was true for the original panel.

Evaluations of Political Figures

Of the original Survey Research Center triad of parties, candidates and issues, all empirical work of a panel type has focused to date on parties and issues, for the simple reason that the 1956–60 period bridged two presidential elections with no continuity of candidates. The same lack of continuity marks the 1972–76 interval. However, the new panel does include multiple evaluations, on a thermometer scale, of the most prominent partisan political figures in the land, even if not running for president. It is naturally of interest to ask how stable these personality evaluations are, by comparison with the high stability of party identification and the relatively low stability of personal positions on political issues. Therefore we shall close our review of some of the first panel findings with a brief consideration of these raw continuity results.

Ratings of Hubert Humphrey, Ted Kennedy, Scoop Jackson, Richard Nixon and George Wallace were drawn in 1972, 1974 and 1976. In addition, a rating of George McGovern was applied in 1972 and 1976. Over all instances, the 100-point thermometer scale was explained and then names were offered *seriatim* as stimulus words for evaluation. The continuity correlations available for these political figures are summarized in Table 1. A number of provisional observations about this array are fairly obvious.

Table 1. Continuity Correlations in the 1972–76 Panel Six Major Political Figures

	Continuity Correlations	
	Average, 2-Yr. Ests.	4-Yr. Ests.
Edward "Ted" Kennedy	.722	.671
George Wallace	.683	.609
Hubert Humphrey	.592	.531
Richard Nixon	.585	.461
George McGovern	–	.554
Henry "Scoop" Jackson	.445	.343

Source: 1972–74–76 panel study of the national electorate carried out by the Center for Political Studies, University of Michigan.

One is that the levels of continuity correlations across six of the most prominent political figures of the era covered by the new panel show a very wide range of variation. While it is not perfectly certain in what currency such comparisons should be made, we find it reasonable to square the above correlational values and speak in terms of the proportions of temporal covariance involved in the various evaluations. In this currency, evaluations of Kennedy appear some 2½ to 4 times more stable than evaluations of Jackson. Given such variation, it is tempting to try to explain its sources. One hypothesis might be that the stimulus words "Henry 'Scoop' Jackson" are more poorly worded as question items than the stimulus words "Edward 'Ted' Kennedy," and that hence the Jackson item is simply less reliable. A plausible rival hypothesis, and one that we considerably prefer, is that more American voters bring more meaning to the Kennedy name than to the Jackson name, have more crystallized attitudes toward it as a stimulus, and hence respond more stably to it. The apparent reliability is not, by this construction, a function of the measuring instrument taken in a vacuum, but rather a product of the interaction between item content and the variable degree of meaning this content has for various citizens.

However, a second observation helps to keep us from considering these continuity correlations too woodenly as though they were test-retest reliability coefficients. Although it may not dazzle the unaided eye, the pattern of continuity coefficients for evaluations of Richard Nixon are quite unique relative to all other contents of the new panel discussed to date. We know from the panel marginals (as if we needed them!) that true change in evaluation of Richard Nixon between 1972 and 1974 or 1976 was of massive proportions. In 1971, according to the Gallup Poll, the American people considered Richard Nixon the most admirable person in the entire world. His thermometer ratings in the 1972 wave of the panel confirm this level of adoration, since he received a mean rating of 65–66 in that year, relative to a rating of 52 or 53 for popular figures like Kennedy or Humphrey. By 1974 he had tumbled into the high 30's, and by 1976 to 31.

Thus there is a major injection of real change in the Nixon case, although as we have seen, it may or may not have been a type of change that affects the stability of relative individual positions. That is, it remains possible that people who were wildly enthusiastic about Nixon in 1972 merely were reduced to lukewarm feelings about him by 1976; and those just mildly warm were reduced to cool feelings, etc., such that there was little leap-frogging of individual positions. What is peculiar about the Nixon pattern, however, is that a good deal of true change (in the latter sense) was touched off by the Watergate drama. If we make the relevant Heise calculations for the Nixon case, we find that quite uniquely relative to all other panel materials, true change competes on nearly equal terms with unreliability in explaining why the raw continuity coefficients are as low as they are.

A final observation concerning Table 1 returns us to the main themes of this essay. We introduced the panel data on personages by way of asking where "candidates"—broadly construed—fit in the hierarchy of continuities defined up to now by parties and issues, given prior panel material. If we are correct in our interpretation of the reasons why Kennedy evaluations are so much more stable than those for Jackson, then it should be clear that we could generate variations in these continuity coefficients for political leaders almost at will, by playing with the known prominence of such figures. In the limiting case, we could probably generate a continuity coefficient approaching zero with a stimulus like "Senator Zablocki," although it is reasonable to imagine that a persistent "ethnic vote" might keep such coefficients from actually touching zero.

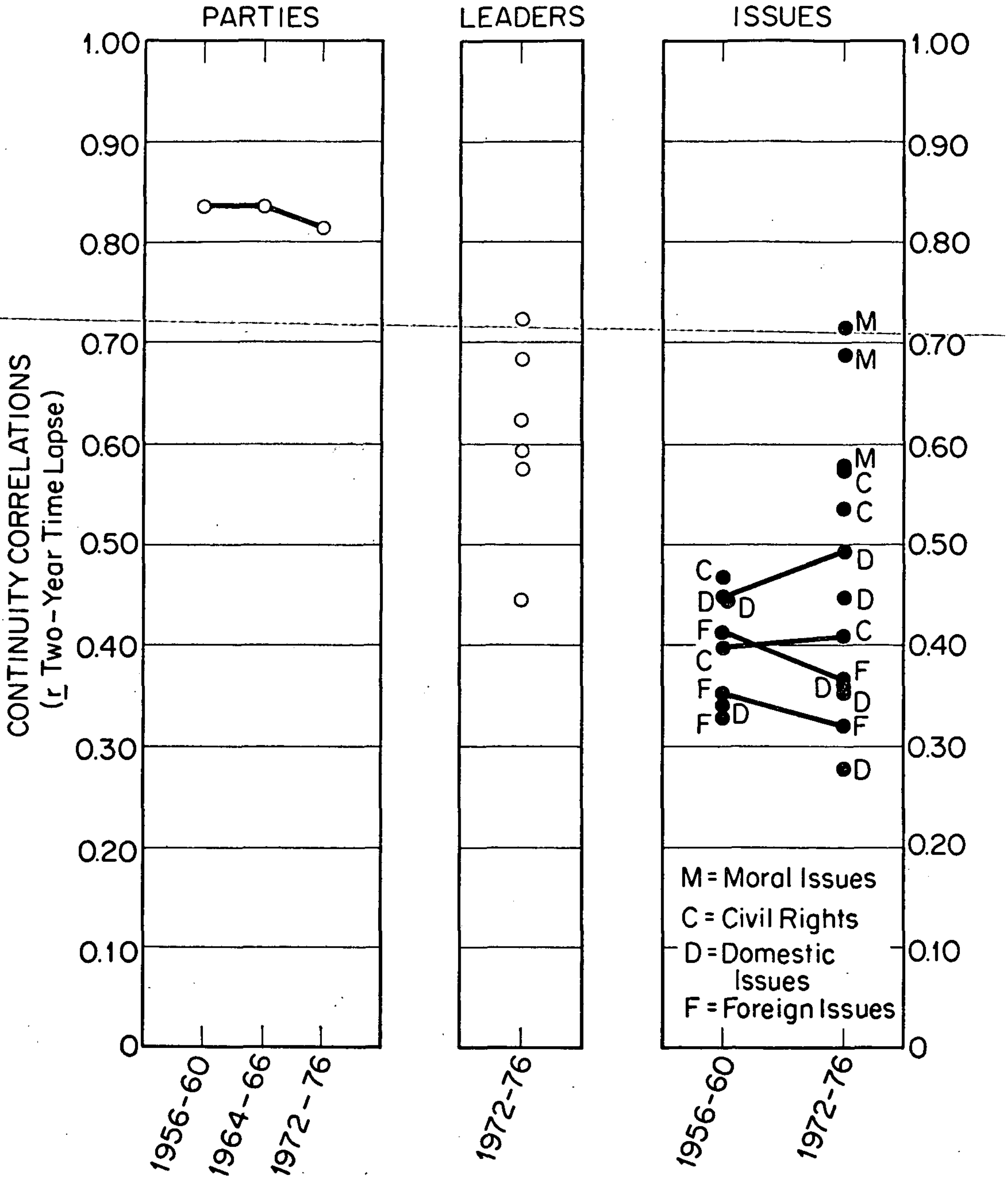
Nevertheless, while it would be a chimera to imagine that some restricted range of continuity coefficients could reliably encompass any political figures whatever (by comparison with parties or issues), it is reasonable to limit our attention to the few truly prominent (in a mass-public sense) political personages in the land, just as the authors of the voting surveys try to restrict their issue measurements to the most salient policy cleavages of any particular period. And if we make such a limitation, then the data in Table 1, coupled with those cited earlier for parties and issues, might strongly suggest that in the hierarchy of continuity that these panel data help to establish, evaluations of major political leaders might properly be wedged in between parties as the most stable objects of orientation in terms of relative voter positioning, and issue positions as the least. Thus major political personages in any given epoch may serve as important cynosures of attention for the electorate, and as significant cues for other more substantive forms of political evaluation.

Summary and Implications

Figure 1 provides a handy graphic summary of the continuity correlation information that we have reviewed. In a nutshell, this summary says that apart from the emergence of some political cleavages having to do with traditional moral values in the later period, there has been scarcely any change in the comparative continuity of party and issue positioning between the two eras, despite manifold reasons to expect not only change, but change of major proportions. The individual-level continuity of party attachments continues to dwarf that for issue positioning in the 1972–76 period, as it did in 1956–60. And affection and dislike for the most prominent political leaders of the epoch seem to show levels of continuity which—again absent the moral issues—clearly lie between parties and issues.

The display in Figure 1 is rich with implications, some few of which we shall develop here. First, however, let us consider an apparent implication or two that should *not* be drawn, particularly with regard to the surprising information concerning the continuities in party identification. We have emphasized before that these continuities do not mean that evidence of weakening partisanship, as registered by change in the party identification marginals since 1965, is somehow imaginary or meaningless. It should be kept clear that Figure 1 does not even address this facet of change in any direct way, even though it is very tempting to confuse the two.

Thus, for example, one improper implication to be drawn from these data is that since the individual-level continuity for panel respondents remains high while partisanship is weakening, it must mean that the weakening is all attributable to new entrants into the electorate who do not figure in a long-term panel table. Hence it would seem that one major point to these data is that the weakening can be traced entirely to population replacement processes. However, such a conclusion again involves a subtle confusion between the two facets of change. Clearly generational replacement has made a major contribution to the weakening of aggregate partisanship, especially in the later portions of the past ten years. But with equal clarity, persons already in the electorate before the decline began have also made further important contributions to it, and have continued to do so all the way to 1975 (Converse, 1976). Such persons do figure in our panel tables, and indeed they show a weakening of partisanship even within those tables: over all segments, including 1964–66, 1965–73 and



Source: 1972–74–76 panel study of the national electorate carried out by the Center for Political Studies, University of Michigan.

*Where items can be plausibly matched between panel segments, the respective data points are linked by a trend line.

**The whole figure is expressed in the currency of *two*-year continuity correlations. Where only four-year correlations are available, plausible two-year interpolations have been made to permit inclusion of a maximal number of observations.

Figure 1. Continuities in Public Response to Parties, Leaders and Issues*

1972–76, later observations on these people show lesser variance in partisanship than earlier ones, although continuity levels remain intact.

Thus the decline in reported strength of party identification over the past decade remains an important but virtually independent fact. Moreover, there is evidence that defection rates for given levels of identification strength have been higher in recent years than they were in the 1950s. Both changes attest to a weakening of partisanship in the electorate. Both are thoroughly documented, and both have had real effects on the operation of the American political system.

For many analytic purposes and prognostications, the weakening of partisanship can be more important information than the near-constancy of continuity correlations summarized in Figure 1. For other purposes, however, the reverse is true, and it remains for us to suggest what some of the genuine implications of Figure 1 would seem to be.

Some of these implications are relatively technical or methodological. Thus for example, the kind of cycling of coefficients over time periods that we carried out with respect to party identification on page 39 makes sense only if the facet of change indexed by the continuity coefficients is essentially stationary, as we have found it to be. More generally, the virtual lack of period dependency of the kind of estimates we have generated from the panel can provide a potent assist for a variety of structural modeling ventures aimed at sorting out in a more satisfactory way the causal interplays between evaluations of parties, issues and candidates. To be carried out effectively, such enterprises presume knowledge of things like measurement reliability. Since such information is rarely available, investigators often plug in quite fanciful guesses in order to proceed. The panel data in themselves provide better-grounded estimates. And the fact that these estimates seem quite invulnerable to changes in period—even when one would least dare to expect them to be, as in the case of partisanship, 1964–76—can be an important liberating fact for the conduct of further research.

The findings have provisional implications for research methods which are broader still. For some time, the cost-benefit ledger for panel designs, as opposed to less expensive independent replications, has been subject to some dispute. One can learn a good deal about important facets of change without making the extra investment in a multi-wave panel. However, other facets of change, such as those central to this report, can only be observed with the help of a panel. One swallow does not a summer make, and it would obviously be premature to conclude that the stationarity of continuity coefficients which marks these political data would emerge for panel studies in all periods with any subject matter. On the other hand, these continuities seem so pervasive across a variety of types of variables in the election panels that the hint of universality is hard to miss. And if there is some generality to the findings, then the premium that might be placed on doing panels persistently over a series like the election studies is at least somewhat reduced, although occasional panel links would remain important.

The findings also have strong bearing on various substantive questions. For example, one of the points of most vigorous theoretical ferment in the past decade in electoral politics has involved the nature of partisan realignments. The subject has captured uncommon scholarly interest not merely because of its obvious importance in any theory of elections, but also because of the seeming imminence of such a realignment after 1964 or so. Given past periodicities, the time was ripe. Much more to the point, several key ingredients associated with past realignments had begun to emerge.

Very crudely put, the most important of these ingredients was the outbreak of uncommonly galvanizing political turmoil, of the sort which certainly marked the late 1960s and early 1970s. More finely drawn, we have the excellent effort of Sundquist (1973) to reconstruct the internal dynamics of a partisan realignment, with stress upon crucial emergence of important cross-cutting cleavages which the leaderships of the extant traditional parties bound to older cleavage lines, fail to articulate. Again, such ingredients were present, par excellence, by the late 1960s. That these new cleavages were gripping for common voters is attested to at least indirectly by the high continuity values associated in the new panel with items like busing and the life-style or moral issues. That these new issues posed cleavages which were truly cross-cutting (i.e., relatively orthogonal to or uncorrelated with prior lines of party alignment) is clearly demonstrated by Miller and Levitin (1976, p. 82), who show the lack of much association between voter persuasions on the newer issues and their party identifications.

One must insist that the new issues be cross-cutting ones to set the stage for a realignment, since otherwise the emergence of new issues more or less aligned with old ones would merely reinforce existing party differences. In the late 1960s and the early 1970s, some

Democrats were affronted by counterculture trends while others were not; likewise, some Republicans approved while others disapproved. What is important about these differential within-party affinities is that if potent enough, they should produce changes in partisanship of a leap-frogging kind, disturbing the relative ordering of individuals on the continuum of partisan loyalties. However, it is exactly this possibility of leap-frogging that our continuity correlations are admirably designed to register, however insensitive they may be to other forms of change. And they give us the message that the leap-frogging type of change in partisanship was scarcely more prevalent over the whole of this period than it had been in 1956–60. Hence while the emergence of cross-cutting cleavages seems to have been real enough, it also seems clear that they did not produce the relevant changes in partisanship in this period, even of the compensating kind which leaves the net balance of partisanship in the aggregate unaffected.

Another set of implications flows from the panel data patterns of the issue items. Our original theorizing led us to imagine that constraint among belief elements measured at a point in time and the stability of those beliefs for individuals across time would tend to covary, although no empirical test of this assumption between periods of time has been possible until the new panel. In effect, if Nie's (1974) demonstration of increased constraint in the recent period has substantive merit, then we would be obliged to expect the stability of these types of items to advance in the new panel as well.

The fact that no such increase is observed poses an interesting problem. One possibility is that the original assumption, which sees variations in attitude crystallization as producing variations both in constraint and in stability, is simply wrong and needs to be rethought. Another possibility which, as we have seen, has already been raised (Bishop et al., 1978; Sullivan et al., 1978) is that the Nie displays are largely a product of changes in question format, rather than true increases in constraint. At first glance, the accrediting of such challenges might seem to solve the problem by removing the weight of the expectation that issue stabilities should have advanced in the new panel. However, even if the Nie results were in fact largely due to question format, one might still expect comparable variations in stability on the same base. Now we saw as we passed through the issue items that small changes in continuity values did appear to follow in expected ways along lines of shifts in question format. However, these latter tests were few and at points equivocal, so that not a great deal should be made of them. But in any event, even these changes were of very limited magnitude. Thus some fundamental problems remain which challenge further research, at least some of which can be carried out with other probings of the new panel data set.

A final cluster of implications which is of great theoretical importance flows from the hierarchy of continuities across parties, leaders and issues suggested in Figure 1, and is relevant for controversies over the prevalence of issue voting. It is not hard to imagine that attitudes which show higher individual-level stability have causal primacy relative to less stable attitudes. If this simple surmise is meaningful, then it would seem that where political evaluations are concerned, a party → leader → issue flow may be dominant, even across periods as disparate as those before and after 1960.

It must be kept in mind that the new panel data are merely suggestive in this regard, rather than definitive. Even the gross discrepancies in continuity between partisanship and issue positioning cannot guarantee that party identification is nearly always causally primary when party feelings are discovered to be aligned with issue preferences. Among other things, we must remember that there are only two major parties, whereas the number of political issues which may exercise one or another voter is legion. Thus it would always be possible, if indeed a bit strained, to imagine that the typical voter might be viscerally gripped by one issue out of some 30 in the active universe in some period, with very uncrystallized attitudes on the other 29. If party positions on such crucial but idiosyncratic issues remained constant over long periods of time, these policy preferences might totally dictate party identifications, at the very same time that measurement over a dozen or so of the 30 potential issues might show very high aggregate instability, since only one voter in 30 would, on the average, be gripped by each.

While such a model remains conceivable, it is obviously not the most plausible reading of current data. The huge discrepancies in individual-level continuities between party loyalties on one hand and issue positions on the other, particularly in view of what now seems to be their long-term constancy, argues for an overwhelming primacy of the party term when party-issue congruence does occur. However, further causal modeling work with the new panel, profiting from some of the kinds of estimates presented in this report, can mold more incisive conclusions from what must remain only suggestive ones at this point.

The additional fact, also made clear by the new panel data, that reactions to political leaders display levels of continuity intermediary between partisanship and policy preferences, raises a number of further interesting possibilities. Such a data configuration may, for example, suggest a significant margin for policy leadership available to major political figures, whereby admirers are quite susceptible to influence leading them to adopt policy positions more congruent with those espoused by their heroes. Or again, the greater continuity in evaluation of leaders as compared with issue positions may simply reflect the fact that party attachments anchor *both* leader assessments and issue positions, but do so more effectively in the case of leaders because the common voter can maintain much more firm and unequivocal cognitive links between parties and their most prominent leaders than between parties and positions on various issues.

This is obviously not the place to begin sorting out such possibilities. However, the first-cut results from the new CPS panel are striking indeed. They already rule out some constructions which might be placed upon the electoral history of the past decade, and begin to draw into focus a number of new research questions which, with more refined "milling" of the panel data, we may well be able to answer effectively.

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Comparisons between Category and Magnitude Scaling of Political Opinion Employing SRC/CPS Items*

MILTON LODGE
BERNARD TURSKY
State University of New York, Stony Brook

The measurement technique most commonly used by political scientists for determining the direction and intensity of opinion is category scaling—a procedure with serious weaknesses. Recent developments in psychophysics for the magnitude scaling and validation of sensory continua offer a powerful alternative to category scaling. Paralleling explicitly the logic and procedures used to scale psychophysically such variables as the loudness of sound and brightness of light, research methods now make it possible and feasible via a simple paper and pencil technique to obtain accurate, precise, cross-modally valid, magnitude measures of the direction and strength of political opinion from respondents in a survey setting.

A field survey, pitting category against magnitude measures for a sampling of the most important items employed in the SRC/CPS national election studies, demonstrates that the category scaling of political variables results in (1) the loss of significant portions of information and on occasion (2) the misclassification of respondents. The results of this scale-confrontation study demonstrate the superior utility of magnitude over category scaling for the description and quantitative analysis of political judgments and preferences.

The measurement technique most commonly used by political scientists for determining the direction and intensity of public opinion is category scaling—a procedure whereby a respondent rates an item or expresses a judgment by selecting one of a fixed number of options. Category scaling, which extends back to at least 150 B.C. when Hipparchus used a six-point scale to judge the brightness of stars, employs a variety of formats. The SRC/CPS, for example, incorporates 2-, 3-, 4-, 5-, 7-point, and thermometer-type category scales in the national election studies. However, despite its long history and widespread contemporary use throughout the social sciences, category scaling as a procedure for measuring strength of opinion has a number of serious weaknesses.

First of all, information is often lost through the limited resolution of the categories. Even when respondents can clearly distinguish between two items, category scaling forces them to make similarity-difference judgments and place items judged to be more or less alike in the same category. The greater the discrepancy between the true range of stimulus intensity and the fixed range of the category scale, the poorer the resolution of the categories. For this reason categorical judgments may be thought of as qualitative rather than quantitative.

A second problem is that category scales represent only an ordinal level of measurement, thereby limiting the statistical procedures which can be used without reservation.[1] Many researchers, not content with ordinal data, assign numbers to the categories, treat them in interval fashion, and use more powerful statistical techniques than are warranted. As Tufte (1969) points out, the assignment of numbers should not be based on an assumption of equal intervals between categories.

And finally, by offering a fixed number of categories, however few or numerous, the investigator is inadvertently affecting the response—artifactually constraining or expanding the range which can be used by the respondent (Baird, Kreindler, and Jones, 1971; Marks, 1968; Stevens, 1975).

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[1] A related problem common to many of the category scales used throughout the social sciences is created by providing a verbal label for each of the categories in the scale. A caution against such a procedure had been voiced by Thurstone and Chave (1929) and Jones and Thurstone (1955), because labels weaken the assumption of equal intervals between the categories and because of the increased variance at each category as a result of differences in semantic interpretation of the label. See too, Lodge et al. (1975) showing that such popular labels as *good-bad, agree-disagree,* and *strong-weak* are not equally spaced.

These problems—inherent in the very act of partitioning an arbitrarily fixed continuum—can be overcome (or more correctly, by-passed) by using magnitude scaling procedures. Developments over the past 20 years in the theory and methods of psychophysical scaling demonstrate that people can make proportional judgments about the perceived intensity of most sensory continua, judgments discerning enough to meet the demanding assumptions of *log-interval* measurement and preserve ratio relations (Stevens, 1966; Marks, 1974).[2]

Magnitude estimation is the simplest, most direct, and currently the most widely used method of constructing scales having ratio-preserving properties. In the classical sensory psychophysical experiment, subjects are presented with a series of test stimuli (say, varying tones, lights, or weights), one at a time in random order, and are instructed to match numbers proportional to the perceived magnitude of each stimulus relative to a reference tone, light, or weight. When these numeric estimates for each stimulus are averaged by geometric means and plotted against actual physical stimulus intensities on ratio-ruled coordinates, the points characteristically fall along a straight line. A linear function on ratio-ruled coordinates entails a power function relationship with the exponent corresponding to the slope of the line. The principle governing this relationship is simple and lawful: *Equal stimulus ratios produce equal subjective ratios.* This is the essence of the psychophysical law governing the perception of most physical continua (Stevens, 1957, 1961). Power functions, each with a characteristic exponent relating stimulus magnitude to perceived magnitude, have been verified for more than 30 sensory continua, among them the loudness of sound (0.67), brightness of light (0.5), heaviness of hefted weights (1.45), pain due to electric shock (2.0), sweet (0.8) and salty (1.4) tasting substances, force of hand grip (1.7), line length (1.0), and numbers (1.0).

Moreover, it is possible via the *cross-modality matching paradigm* to validate the power functions governing the relationship between physical stimulus intensities and perceived magnitude (Stevens, Mack and Stevens, 1960; Cliff, 1973; Hamblin, 1974). When two independent measures with well-established characteristic exponents—say number or line production, force of hand grip or sound pressure—are matched to a quantitative variable, and plotted one against another, theory predicts a resulting power function with an exponent that closely approximates the established ratio of the two response measures. Suppose, for example, that force of hand grip (*HG*) and sound pressure (*SP*) are used to judge a physical continuum such as weight. Since the established exponent for *HG* is 1.7, and that for *SP* is 0.67, when these two response measures are plotted one against the other on ratio-ruled coordinates, theory predicts a power function with an exponent equal to the ratio between the two exponents (here $HG = SP^{.67/1.7}$). The same test applies to number production (*NP*) and line production (*LP*) measures: since both *NP* and *LP* characteristically produce unit exponents, the ratio of these two magnitude measures to a set of physical stimuli governed by the power law should approximate unity ($NP^{1.0}/LP^{1.0} = 1.0$). When the data from a cross-modality matching experiment produce a good first-order approximation of the theoretical relationship, one has confirmed the psychophysical validity of each measure.

What is more—and this is a major development in the history of social scaling—by employing two magnitude measures with established exponents in the cross-modality matching paradigm, it is also possible to validate magnitude scales of social opinion. That the logic, method, and criteria for validating magnitude scales are the same for both physical and social variables, is illustrated by the study that produced the adjectival support scale (Lodge et al., 1975, 1976a, 1976b). In that scale-building study both laboratory subjects and survey respondents made numeric, hand dynamometer, and sound pressure responses to estimate the strength of support expressed by 30 adjectival scale items (among them, *very good, good, so-so, bad, very bad*) when applied to the United States Senate, Supreme Court, then-President Nixon, the Democratic and Republican parties. This support scale was judged valid when the *NP*, *HG* and *SP* exponents obtained from the matches to the scale words approximated closely the theoretical ratios for each pair of measures.

The cross-modality matching paradigm constitutes a stringent test for confirming or disconfirming the internal validity of magnitude scales. Not only must the empirically derived relationship between exponents result in a power function, but, a still more exacting criterion, the empirical function obtained must match the theoretical ratio between exponents. In the scaling of social variables, where stimulus intensities are not known beforehand and therefore cannot be confirmed directly by plotting them against scale values, this paradigm is especially commanding. When respondents match two independent magnitude measures with established exponents to each social stimulus, the observed ratio between exponents should be identical to the theoretical ratio established for both measures when matched to physical stimuli. Essentially, then, the cross-modality matching paradigm allows an investigator to impose a metric on those social continua which have heretofore unknown underlying metric properties.[3]

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[2] Whereas interval scales are based on the equating of differences ($Y = aX + b$), logarithmic interval scales are based on the equating of ratios ($Y = bX^a$) or ($\log Y = (\log X)(a) + (\log b)$). While not a *true* ratio scale ($Y = aX + 0$) in that the zero point is arbitrary, log interval scales preserve proportionality and, hence, are amenable to higher-order statistics.

One may well be skeptical of adapting methodological innovations in one field of inquiry to meet the felt needs of another. What practical difference, if indeed any, would result from using magnitude instead of category scaling to measure the intensity of political opinions? To address that question squarely, we conducted a scaling confrontation study in the field to produce data necessary for a direct comparison between the magnitude and category scaling of political opinions. We shall (1) describe the procedures developed for incorporating magnitude scaling in survey instruments and the design of the confrontation study, (2) present the results of this study, and (3) discuss the relative costs and benefits of the two scaling methods.

The Magnitude Scaling of Political Opinion in Survey Settings

Paralleling explicitly the logic and procedures developed by psychophysicists for the magnitude scaling of sensory continua, the Laboratory for Behavioral Research has developed and tested in the field a simple paper and pencil technique for the magnitude scaling and cross-modal validation of strength of political opinion in survey instruments (Lodge et al., 1975, 1976a, 1976b). After outlining the general procedures for the magnitude scaling of strength of political opinion, we shall describe the specific methods used in this study.

General Scaling Procedures. Several criteria govern the choice of response modalities for incorporating magnitude measurement in survey research. The response modalities must (1) have exponents which have been well established in the psychophysical literature, (2) provide a large response range, (3) be portable and relatively inexpensive to produce in quantity, (4) be capable of administration by survey interviewers untutored in psychophysics, and (5) be easily utilized in a more or less conventional interview schedule. Two response modalities—line and number production—best satisfy these criteria.[4]

Employing line and number production, the cross-modality matching paradigm involves respondents in two scaling tasks: one, the calibration of the two response modalities, the other, the direct magnitude scaling of strength of opinion. The calibration exercise, taking 5–6 minutes to administer, is a straightforward psychophysical scaling task: relative to a reference, respondents make line and number judgments of a physical metric, a calibrating continuum. The most direct approach is to have respondents draw lines relative to a reference line for a geometric sequence of number stimuli and assign numbers relative to a reference number for an identical set of geometrically spaced line lengths. (See Lodge et al., 1976b, for a description of calibration tests and checks on the use of line and number production.) Procedurally, the calibrating stimuli are presented one at a time, twice each, in random order and cover a broad (here 150:1) range.

In addition to providing training and practice in the use of line and number production in making proportional judgments, the calibration task makes it possible to test each respondent's ability to make proportional judgments. Thus, one can weed out the small percentage of the population (2–5 percent) that for physical or psychological reasons cannot make accurate or reliable proportional judgments. The 95 percent confidence limits for the empirical exponent relating the responses to the calibrating continua should, and typically do, encompass the theoretical exponent. Far more importantly for our purposes, this same criterion must be met by the responses to the political stimuli. Thus, one can be confident that the scale values obtained from the magnitude measures are valid if this stringent test is passed.

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[3] To date more than a dozen social-psychological variables have been scaled using numeric estimation, among those of interest to political scientists: the seriousness of crimes (Sellin and Wolfgang, 1964); prestige of occupations and social status (Hamblin, 1971; Künnapas and Wikström, 1963; Shinn, 1969; Rainwater, 1971); importance of political offices (Shinn, 1969); strength of religious attitudes (Finnie and Luce, 1960); frustration-aggression (Hamblin et al., 1963); national conflict and cooperation (Corson, 1970); and racism (Dawson and Brinker, 1971).

[4] After early research using loudness, hand grip, and number production for scaling public opinion (Lodge et al., 1976a), line and number production were determined to be two response modalities which best satisfied the above criteria (Lodge et al., 1976b). Number production (popularly referred to as magnitude estimation) has a long history in both sensory and social-psychophysics. Line length production has also been used in a number of studies (Shinn, 1969; Stevens and Guirao, 1963; Teghtsoonian and Teghtsoonian, 1965).

The second scaling task, which is preceded by 5–6 minutes of instruction in the use of line and number production to make social judgments, consists of the actual magnitude scaling of each opinion item. This task proceeds in discrete steps for each survey question: (1) after reading the question, the respondents are presented with the polar options which define the dimension (e.g., the labels *Democrat* and *Republican* for the party identification item or *agree* and *disagree* for the conventional efficacy and citizen-duty items) and then asked to select the option that *is closer to . . . agrees with . . . support . . .* or *best expresses one's opinion.* In doing this the respondent indicates the direction of response. Next, (2) the respondents draw a line (a reference line) to represent a middle position between the options, and, then, (3) relative to this reference line, draw another line (a response line) to express proportionately their strength of opinion. On a separate page, (4) the respondent assigns a number relative to this reference to express opinion intensity. Finally (5), if, as here, category scales are also used, the respondent checks the most appropriate categorical option.

The procedure for the party identification question may serve as a model for the way in which questions were responded to psychophysically. Each of the respondents first checked the appropriate position on the standard two-part format to indicate whether they were Democrat, Republican, or Independent. Then, those respondents who indicated they were Republicans or Democrats next checked whether they were *strong* or *not so strong* partisans; or, if Independents, whether they felt closer to the Republican or Democratic party. Next, each respondent drew a reference line (a standard) to represent a middle position between Democrat and Republican. Then, relative to this reference line, the respondents drew a response line to indicate strength of partisanship. So, for those respondents who think of themselves as Independents, their response lines would be approximately the same length as their standard lines. The more partisan respondents felt themselves to be, the longer their response lines would be relative to their own reference lines.

Number responses were made on the following page of the interview schedule without referring to the line-length responses. The same procedure was used; first, each respondent assigned a reference number to represent a middle position between the categorical options Democrat and Republican, and then made a response number relative to the standard number to express the intensity of opinion. Similar procedures were used for all psychophysical responses: after indicating the direction of opinion by making a choice between the polar options provided by the SRC/CPS item, respondents expressed their relative strength of preference by means of lines and numbers.

The construction of magnitude scale values for the responses to each question is a simple procedure. Since two ratios were created for each question, one for the response line to the reference line and one for the response number to the reference number, the function relating the ratios can be examined to determine the degree to which the predicted unity exponent is obtained. The calculation of scale values merely involves averaging the two response ratios to produce a single magnitude measure of response. Thus greater reliance can be placed on the individual-level data, because any bias remaining in either of the two measures is minimized. It cannot be stressed too strongly that it would be impossible to evaluate or minimize any response error were it not for the use of two measures within the cross-modality matching paradigm.

Methods and Procedures for the Scale Confrontation Study. To confront the two scaling methods directly, we administered an interview schedule containing a variety of category formats sampling the most popular items which organize research on American electoral behavior to a purposive sample of 211 local area adults. The items were drawn from the SRC/CPS election studies, as well as from Almond and Verba's *The Civic Culture* (1963) and Verba and Nie's *Participation in America* (1972). Each of the items was presented word-for-word, option-by-option *exactly* as specified in the respective codebooks. Each respondent made categorical, line, and numeric responses to each item, thereby allowing direct comparisons between category and magnitude measures for a variety of well-known 2-, 3-, 4-, 5-, and 7-point category scales.[5]

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[5] Various SRC/CPS versions of the thermometer

The Sample. Two surveys were carried out—one in May, 1976 (n = 103) prior to the New York State presidential primaries, the other in October, 1976 (n = 108) immediately preceding the election. The respondents in both samples were recruited by contacting local community organizations or local chapters of national organizations, including Parent-Teacher Associations, the League of Women Voters, local political party committees, and the American Cancer Society. All 211 respondents were from Suffolk County, Long Island, New York. This purposive sampling provides variability across such conventional demographic variables as sex, age, education, and income, as well as along a political activity dimension. All interviews were conducted by members of the laboratory staff with groups of 5 to 15 respondents in private homes, school cafeterias, and meeting halls. Respondents were paid for their participation in the survey, which took 70–90 minutes to administer.

The Interview Schedules. The May and October survey instruments are similar, containing a total of 28 different items commonly employed in political survey research, including party identification, attention to politics, likelihood of voting, candidate preference, the political efficacy and trust in government indices, as well as the standard items measuring support for such issues as government action on jobs, inflation, school busing, health insurance, and abortion. The interview schedule was printed on legal-sized (8½" x 14") paper to provide adequate length for line length stimuli and responses (see Lodge et al., 1976b).

The Calibration and Construction of Magnitude Scale Values. First, we analyzed the cross-modality matches to the calibrating continua (lines matched to numbers and numbers matched to lines) to determine whether the empirical data yielded the theoretical exponent (1.0 for each modality) that would have to be obtained if the respondents had in fact used both modalities as long-interval metrics. When the numeric estimates are regressed against the line length stimuli and plotted on ratio-ruled coordinates (Figure 1a), the slope is .90; and when the line-drawing responses are regressed against the number stimuli (Figure 1b), the slope is .89.[6] Using averaged responses (as shown throughout Figure 1), the correlation coefficient in each case is .99. Although the regression coefficients are slightly lower than the theoretical for both sets of matches against metric stimuli, the relationship is well described as a power function—a straight line on ratio-ruled coordinates.[7] The effects of regression bias are, in this case, relatively minor, primarily affecting the line production responses, and serve to compress the line length responses only slightly.

When the line and number production responses are plotted one against the other (each being matched to identical number and line length stimulus intensities), one would again expect a slope of 1.0 (Figure 1c). The number production versus line production slope is .91.[8] Again the correlation is .99 for both samples. The correlation coefficients as well as the scattergrams indicate a very high degree of agreement between the two magnitude measures, as well as evidence that the power function fit is very good.

Of prime importance here is that when the cross-modality matches to the metric stimuli result in a good first-order approximation of the theoretical power function governing the relationship between the two modalities, a criterion is established against which the re-

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scale are now being tested in the laboratory. It is anticipated that the thermometer format—despite its apparent 101-point range (98 points when recorded by SRC)—is in fact a 7-, or 9-point scale and thereby subject to all the problems inherent in the partitioning of a fixed continuum. Problematic too is the practice of labeling every 15^0 between *warm* and *cold.*

[6] These correlation and regression coefficients are reliable across both samples: for the May sample, the numeric estimates of line length stimuli yield r = .99, b = .99; for the October sampling, r = .99, b = .94. The line production to number relationship for the May sample is r = .99, b = .88; for the October sample, r = .99, b = .89.

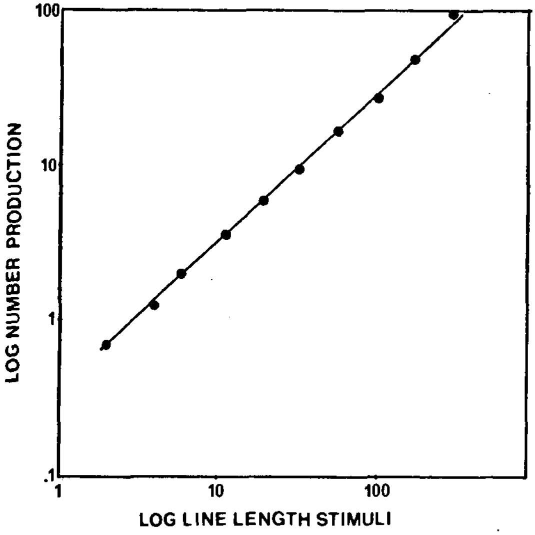
[7] The primary cause for this lower-than-unity relationship is a bias referred to as regression bias (Stevens and Greenbaum, 1966), which is found almost invariably to operate on all response modalities. It is produced by a tendency to overestimate smaller stimuli and to underestimate larger stimuli, with the net result being a slope lower than one would expect under ideal circumstances. Regression bias can be—and is here—corrected (see Cross, 1974; Lodge et al., 1975, 1976a).

[8] The correlation and regression coefficients obtained from the indirect cross-modality match are reliable across the two samples; for the May sample, r = .99, b = .89; for the October sample, r = .99, b = .95.

spondent's subsequent magnitude estimates to the political stimuli can be compared directly. When, as here, the exponents derived from the matches to political stimuli closely approximate the exponents obtained from the matches to the calibrating stimuli, the social scales are judged to be validated.

Results

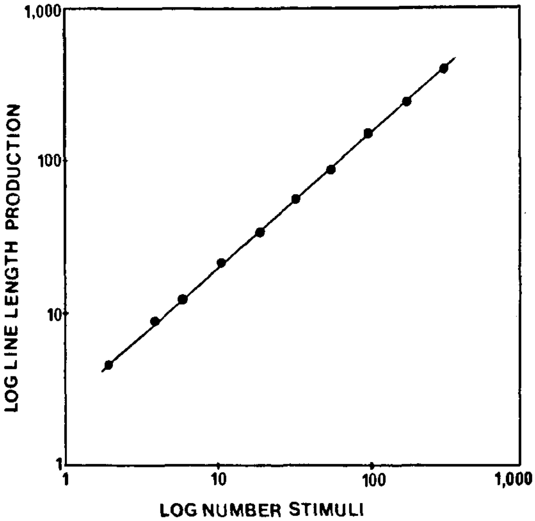
Comparisons Between Category and Magnitude Scales of Political Opinion. The critical assumption underlying category scaling is that respondents can make linear partitions on quantitative continua. Two decades of empirical research involving hundreds of psychophysical experiments demonstrate that human observers have only a limited capacity to make categorical judgments of physical variables which are linear with known values for the continuum. When category and magnitude scales are compared directly in matches against metric values along a physical continuum, the evidence is commanding that category scaling procedures cannot generate equal differences or intervals in data. The relationship between scales is almost invariably curvilinear, most often approximating a logarithmic function (Eisler, 1962, 1963; Galanter and Messick, 1961; Shinn, 1969, 1974).



Source: Data collected from suburban New York sample by the Laboratory for Behavioral Research, SUNY, Stony Brook, May and October 1976 surveys.

Both axes are ratio-ruled and the linearity of the function indicates the goodness of the power function fit. Each point represents an average of 210 respondents from the May and October samples. The slope is .90 and the correlation is .99.

Figure 1a. Magnitude Estimation (Y-Axis) as a Function of Line Length Stimuli (X-Axis).



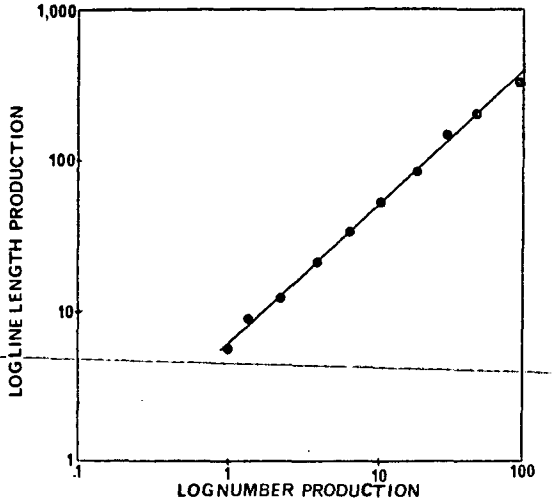
Source: Data collected from suburban New York sample by the Laboratory for Behavioral Research, SUNY, Stony Brook, May and October 1976 surveys.

Both axes are ratio-ruled and the linearity of the function indicates the goodness of the power function fit. Each point represents an average of 210 respondents from the May and October samples. The slope is .88 and the correlation is .99.

Figure 1b. Line Length Production (Y-Axis) as a Function of Number Stimuli (X-Axis).

To the extent the relationship between category and magnitude scales for social-political variables is similar to that found for physical variables (Shinn, 1974; Lodge et al., 1975), we expect that category scales will result in the loss of information. Essentially, the greater the discrepancy between the strength of a respondent's opinion and the response range imposed by the category scale, the greater the loss of information.

The range of opinion expressed by the respondents in this study using magnitude response measures is quite expansive, often as high as 100:1 or more. When this response range is forced into the standard 2-, 3-, 4-, 5-, or 7-point category scales, the results are predictable: the fewer the number of categories offered, the greater the variation of the magnitude responses which must be forced into them.



Source: Data collected from suburban New York sample by the Laboratory for Behavioral Research, SUNY, Stony Brook, May and October 1976 surveys.

Both axes are ratio-ruled. Each point represents an average of 210 respondents from the May and October samples. The slope is .91 and the correlation is .99.

Figure 1c. Indirect Cross-Modality Match of Line Length Responses (Y-Axis) with Magnitude Estimation (X-Axis).

Common sense would lead one to expect differing ranges of response from question to question. With category scaling, however, changes in range of opinion are artifactual, caused by a change in the number of categories dictated by the format of the category scale. Since magnitude scaling provides a relatively unconstrained response range, which varies from question to question, it is highly likely that the magnitude scales will better track the true range of the respondent's opinions.

A review of the scale comparison results for each of the SRC/CPS items incorporated in this study demonstrates that the problems inherent in the category scaling of physical variables apply also to the category scaling of political variables. To illustrate the problems and explicate the reasons for the loss of information, we will examine in detail representative SRC/CPS questions from the survey.

Two- and Three-Point Category Scales: Trust in Government. The five questions used to build the Trust in Government Index (Miller, 1974) were included in the May survey, and illustrate the problems engendered by the use of narrow-range category scales. Subjects responded both psychophysically, using numeric and line production, and categorically, using the procedures prescribed by the CPS codebook, to each of the five questions. Across all five the relationship between the two magnitude response measures is strong, with an average Pearson product-moment correlation of .97, thereby indicating that these respondents were able to make quantitative judgments along each of the distinct dimensions evoked by the individual trust items. A typical scattergram shows the strong relationship between the two magnitude measures, here to the first item, "How much of the time do you think you can trust the Government in Washington to do what is right?" The magnitude range for this question is from 100 times neutral in the distrusting direction to 8 times neutral in the trusting direction. An examination of the magnitude values indicates a fair amount of distrust, with a mean for the sample of 1.6 times neutral in the distrusting direction for this particular question.[9]

Figure 3 displays how the conventional 2- and 3-point scales elicit opinion intensity. In Figure 3 the mean magnitude scale value (± one standard deviation either side of the geometric mean) for each of the trust items is plotted as a function of the categorical position chosen by the respondents. The range of opinion expressed in magnitudes on each of the trust items exceeds by far the absolute range imposed by the category scales. Consequently, the variation within categories is large, with average standard deviations around log-mean magnitude values of .337 for 2-point scales and .296 for 3-point category scales. An example of the problem is readily visible in the responses made to the first item where respondents who chose the least trusting response offered (*Only some of the time*) range from those who are neutral to those who express strong distrust. This broad range of opinion intensity within each category is typical of each question used to construct the Trust in Government Index (as well as the Efficacy and Citizen Duty indices) and invariably results in the loss of information. The problem—inherent in the very act of categorizing a continuum—is so pervasive that it cannot be rectified by construction of an index.

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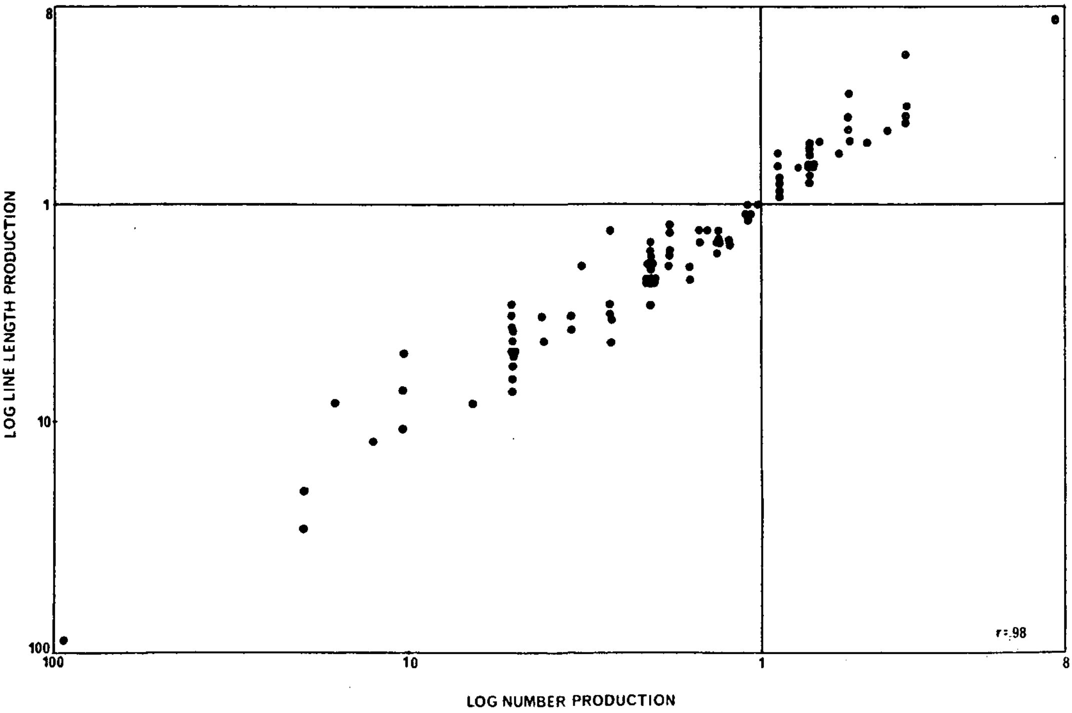
[9] This pattern is found for three of the four remaining questions, the sole exception being the fourth question which asks, "Do you feel that almost all of the people running the government are smart people who usually know what they are doing, or do you think that quite a few of them don't know what they are doing?" On this question the mean scale value is at the neutral point, so the sample is evenly divided. However, for the fifth item, the respondents indicate there are a lot of crooked people in government (with a mean scale value about two times neutral in the distrusting direction), so they evidently believe that there are a number of smart and crooked people in government.

As is also clear from Figure 3 (and from similar plots for the efficacy and citizen duty items) there are instances where the categorical options misclassify respondents. For example, in the third item which deals with the waste of tax money, none of the available options appears to represent trust. *Waste a lot* and *waste some* are, not surprisingly, negative, while *Don't waste very much* appears to be a neutral point—perhaps a standard for governmental performance. A similar problem shows up on the fifth item which focuses on crooks in government, where—in addition, perhaps, to an instance of scale intransitivity—there is no categorical option available to balance the most distrusting category *Quite a few*.

The poor resolution of categories, resulting in the loss of information (and occasionally the misclassification of respondents) is characteristic of all 28 items included in this scale-confrontation study. In addition to introducing large portions of error into our measurement, the reliance on category scaling makes it exceedingly difficult to describe opinion distributions precisely, specify amounts or degrees of opinion change, or test quantitative hypotheses. We will return to this assessment in the conclusion, but let us introduce the problem here specific to the most straightforward of the trust items: "How much of the time do you think you can trust the government in Washington to do what is right?" This item includes three options to tap trust-distrust for the government in Washington—*Just about always, Most of the time,* and *Only some of the time. Just about always* and *Most of the time* are both positive



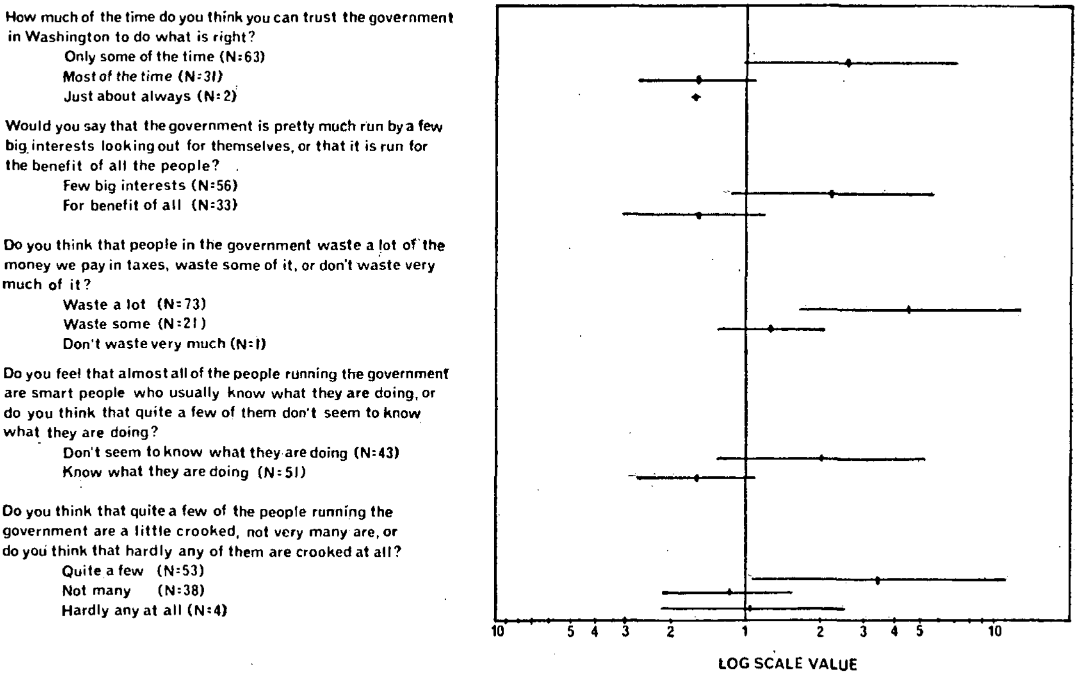
Source: Data collected from suburban New York sample by the Laboratory for Behavioral Research, SUNY, Stony Brook, May 1976 survey.

The variable being judged is the first question used in the construction of the Trust in Government Index—"How much of the time do you think you can trust the government in Washington to do what is right?" The axes are logarithmic coordinates, and the slope is 1.0 with a correlation of .98. Visual inspection shows the strong relationship between the two response modalities.

Figure 2. Scattergram of Line Length Production (Y-Axis) with Magnitude Estimation (X-Axis) for the May Sample.

(i.e., "trusting") responses, while *Only some of the time* is hardly a very negative statement—surely not as negative as statements which more closely parallel the two positive statements, such as *Most of the time I don't trust the government* or *I just about never trust the government.* The three categories offered are not equally distanced from neutral and are skewed in a positive direction. If one could ignore the question as to whether respondents were expressing trust for incumbents or regimes (Citrin, 1974), and found that respondents chose the category *Only some of the time* with increasing frequency over time, then it would seem reasonable to speak in general terms of declining trust among respondents. However, since the responses offered did not allow respondents to express low levels of trust, this decline might well have been from an extremely high to a moderate level of trust. To state that such a decline indicated widespread discontent among the population would be reading far more into the data than is warranted. The relative growth or decline of trust cannot be determined with category scales as they are currently used in survey research because they are not quantitative measurement techniques.

Seven-point Category Scales: Issue Preferences. The seven-point category scale which the Center for Political Studies has recently begun to use to measure strength of opinion to various issues represents distinct advantages over the 2-, 3-, and 4-point category scales used in the Trust in Government, Efficacy, and Citizen Duty indices, and represents the most stringent test available of the magnitude scaling of political opinion. In point of fact, however, these category scales are not seven-point scales in a true sense, but rather two 4-point scales joined together: ostensibly, a respondent has only four choices of response available—a neutral position or three degrees of support for an issue position. So, while the sample as a whole can range over the entire seven categories, each respondent has but four categories available. This format represents the most expansive response range offered in conventional surveys.



Source: Data collected from suburban New York sample by the Laboratory for Behavioral Research, SUNY, Stony Brook, May 1976 survey.

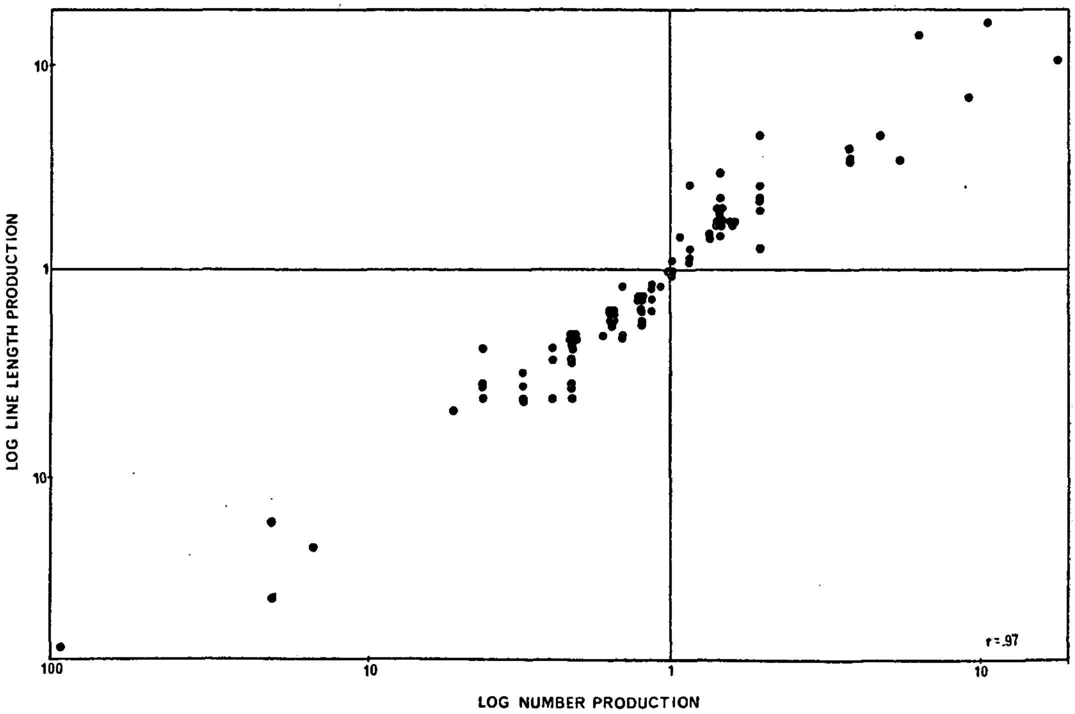
The X-axis is ratio-ruled. Psychophysical values are shown as a mean plus/minus one standard deviation.

Figure 3. Psychophysical Responses (X-Axis) for each of the Category Levels of the Questions Comprising the Trust in Government Index Found in the May Sample.

In the May sample, the issue questions dealt with busing, abortion, taxes, health insurance, pollution, inflation, marijuana, and the provision of federally funded jobs. In the October sample, the questions relating to marijuana and pollution were left out to conserve time. For the May sample, the correlation between the two magnitude responses for each issue was, on average, .94, while for the October sample it was .89.

Figure 4 plots line against number estimates on log-log coordinates to display a typical relationship found between the two magnitude responses to the issue items, in this example strength of support or opposition to government-guaranteed jobs. The Pearson product-moment correlation between number and line length expressions of support is strong (.97), implying a high degree of linear dependence between log measures, as predicted by the underlying psychophysical theory because of the functional relations which hold for these magnitude measures.

Figure 5 shows the relationship between the magnitude and category scale values averaged across all 14 issues to which the respondents in both samples expressed an opinion. As is the case for each and every item, we find the greatest amount of variance in the endmost categories. The variance at the categorical endpoints is a consequence of the category scale's inability to map a sufficiently large range of opinion. On average, from (geometric) mean to mean, the magnitude range across all issues is 40:1. The full range of opinion was much greater—exceeding 100:1 on some issues. The questions dealing with abortion and school busing elicited the most intense responses with ranges from mean to mean of 163:1 and 100:1, respectively. Such intensity information is completely lost when one relies upon categorical judgments of opinion. With category scales, the



Source: Data collected from suburban New York sample by the Laboratory for Behavioral Research, SUNY, Stony Brook, May 1976 survey.

The variable being judged is the first issue which the respondents judged, dealing with whether or not the government should guarantee jobs for individuals. The relationship is shown in logarithmic coordinates.

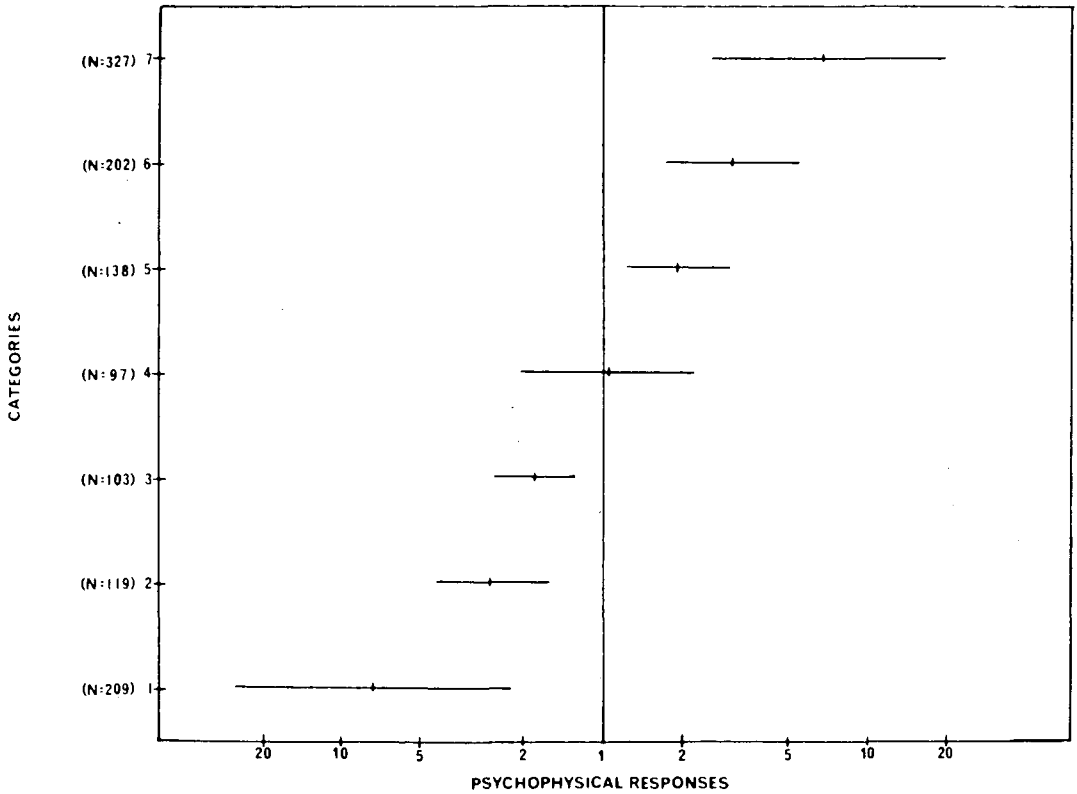
Figure 4. Scattergram of Line Length Production (Y-Axis) with Magnitude Estimation (X-Axis) for the May Sample.

only possible means of determining differences in strength of opinion would be through an analysis of the percentage of respondents who chose the particular categories on the assumption that the endmost categories would be chosen more frequently on issues for which there is the greatest affect. The problem—as visually displayed in Figure 5—is that the variance within each category, especially the end ones, includes respondents who vary substantially in their intensity of support or opposition.

Seven-Point Category Scales: Party Identification. A second set of examples illustrating how category scaling results in the loss of information can be illustrated by examining the 7-point category scale (culled word-for-word from the election studies) to measure party identification. The two-part party identification question was administered to both samples in both a categorical and psychophysical fashion.

First, an examination of the relationship between the two magnitude measures for both samples combined reveals a strong product-moment correlations of .91 (.97 for the May sample and .90 for the October sample). The full range of partisanship is over 60:1. The mean magnitude range from Democrat to Republican is about 15:1 for both samples. It is a consequence of the strength of partisanship evoked by the party identification question which, when arbitrarily constrained on a seven-point category scale, produces the loss of information and misclassification of respondents typical of category scaling.

When the magnitude measures of party identification are viewed as a function of the



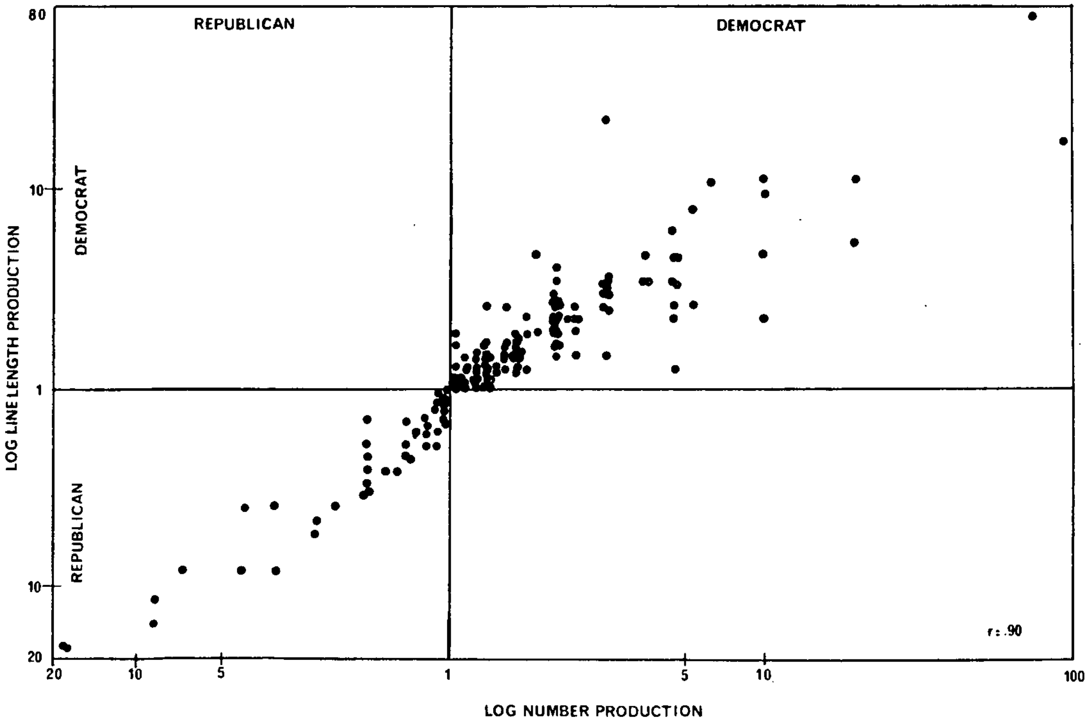
Source: Data collected from suburban New York sample by the Laboratory for Behavioral Research, SUNY, Stony Brook, May and October 1976 surveys.

Psychophysical responses are shown as a mean plus/minus one standard deviation. The X-axis is ratio-ruled. The increased variation at categories 1, 4, and 7 should be noted.

Figure 5. Average Psychophysical Responses for All Fourteen Issues Combined (X-Axis) at Each of the Seven Categories (Y-Axis) from Both the May and October Samples.

categorical responses, one observes a pattern similar to that uncovered for issue preferences. Inspection of Figure 7 makes it clear that there is a broad range of partisanship within each of the categorical levels. What is again obvious is that the greatest amount of variance occurs in the endmost categories, indicating that the most partisan respondents are the most constrained by the categorical response. Further, it is clear from Figure 7 that *Leaning* and *Moderate* partisans are closer to one another than are the moderate and strong partisans. In fact, it is impossible to distinguish psychophysically between those who placed themselves in either the *Leaning* or *Not so strong* categories. The failure of middle categories to distinguish between stimuli is characteristic of many of the 7-point category scales examined in this study, although we know of no measurement-theoretic explanation of the effect.

Candidate Preference as a Function of Party Identification. Another item which appeared in both samples dealt with candidate preference—specifically whether the respondent favored Gerald Ford or Jimmy Carter. Figure 8 plots the relationship between candidate preference and party identification for the May sample with both variables measured psychophysically. The correlation between variables is .83, but more importantly it is possible to examine the slope of the relationship. The best-fitting function relating candidate preference to party identification for the May sample is a power function with a slope of 1.4: a twofold increase in party identification produces a 2.6 increase in candidate support. For the October sample (Figure 9) the correlation is again high, .81, with a power function exponent of 2.0. Here, in the weeks immediately preceding the election, party identification produces a greater



Source: Data collected from suburban New York sample by the Laboratory for Behavioral Research, SUNY, Stony Brook, May and October 1976 surveys.

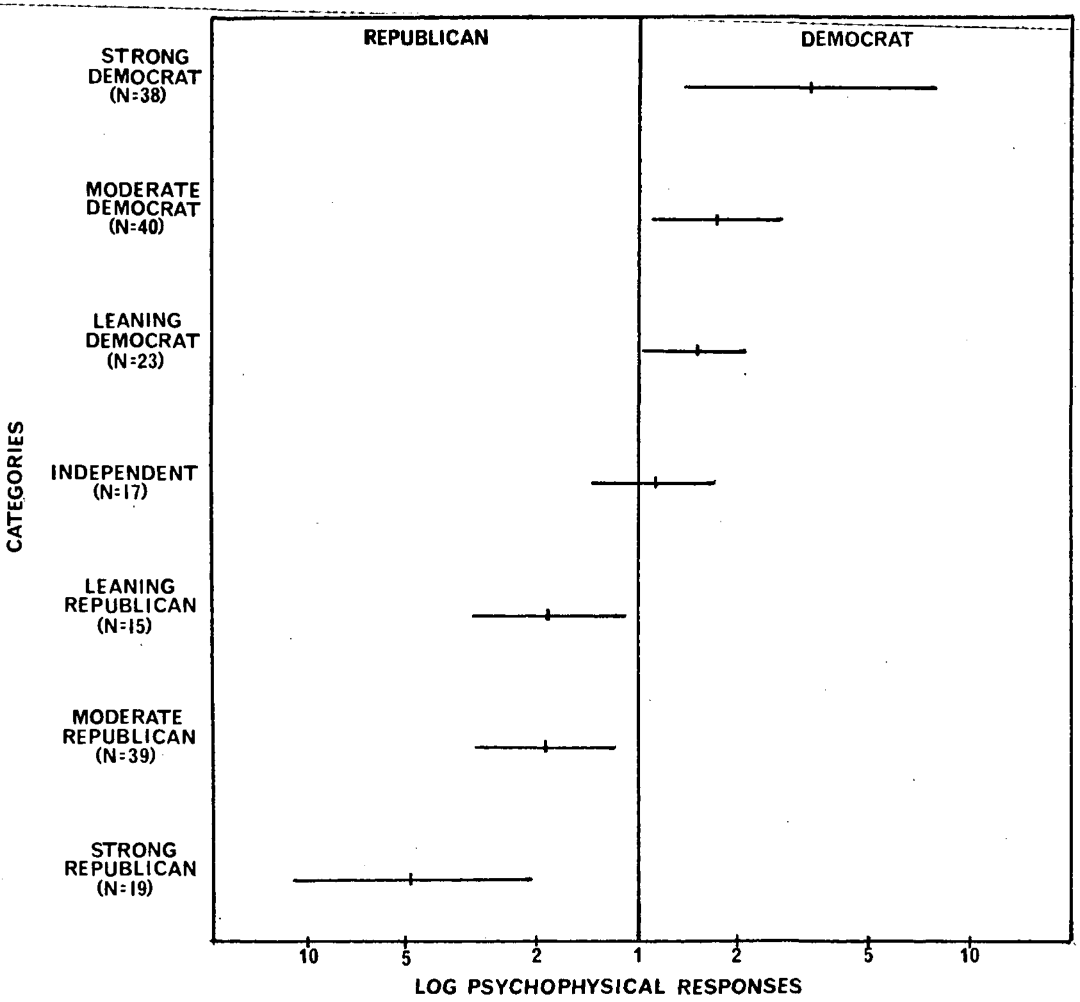
The variable is the party identification question. Again, the strong relationship between the two measures is obvious. The relationship is shown in logarithmic coordinates.

Figure 6. Scattergram of Line Length Production (Y-Axis) with Magnitude Estimation (X-Axis) for Both the May and October Samples.

change in candidate support: a twofold increase in party identification produces a fourfold increase in the likelihood of voting for that party's candidate.

Discussion

When, in sensory psychophysical scaling experiments, magnitude and category scales are compared—either directly by plotting each scale-type against known physical values or indirectly by plotting one scale against the other to a common metric—magnitude scales are almost invariably found to be superior (Shinn, 1974; Marks, 1968, 1974; Eisler, 1962; Stevens and Galanter, 1957). The results of this scale-confrontation study demonstrate that the same problems which characterize the category scaling of physical stimuli appear when respondents express their political opinions categorically. Specifically, the category scaling of political judgments results in (1) the loss of information, (2) ordinal level response data, (3) the



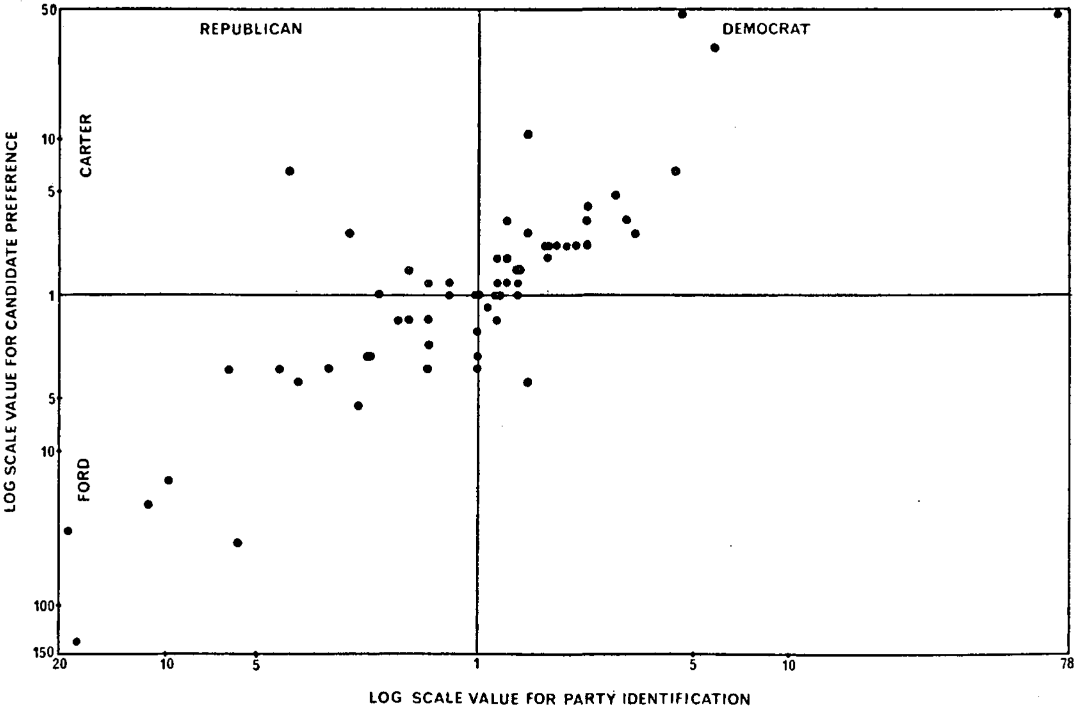
Source: Data collected from suburban New York sample by the Laboratory for Behavioral Research, SUNY, Stony Brook, May and October 1976 surveys.

Psychophysical responses are shown as a mean plus/minus one standard deviation. The X-axis is ratio-ruled.

Figure 7. Psychophysical Responses (X-Axis) for Each of the Party Identification Category Values (Y-Axis) for Both the May and October Samples.

misclassification of respondents, and (4) the arbitrary assignment of numbers to categories and indeterminate regression coefficients.

Magnitude scaling, on the other hand, offers distinct advantages, *but not without costs.*[10] First, given proper instruction in the use of magnitude measures to make proportional judgments,[11] magnitude scaling is isomorphic with the way people make judgments about quantitative variables, both physical (Cliff, 1973) and social (Stevens, 1972). Secondly, magnitude scaling produces log-interval measures of opinion strength, thereby providing researchers with legitimate access to powerful statistical tools. Thirdly, magnitude scaling places no investigator-imposed constraints upon the respondent, thereby enabling respondents to express and



Source: Data collected from suburban New York sample by the Laboratory for Behavioral Research, SUNY, Stony Brook, May 1976 survey.

Both variables are measured psychophysically and the relationship is plotted in logarithmic coordinates. The slope is 1.4 and the correlation is .83.

Figure 8. Candidate Preference for Ford vs. Carter (Y-Axis) as a Function of Party Identification (X-Axis) for the May Sample.

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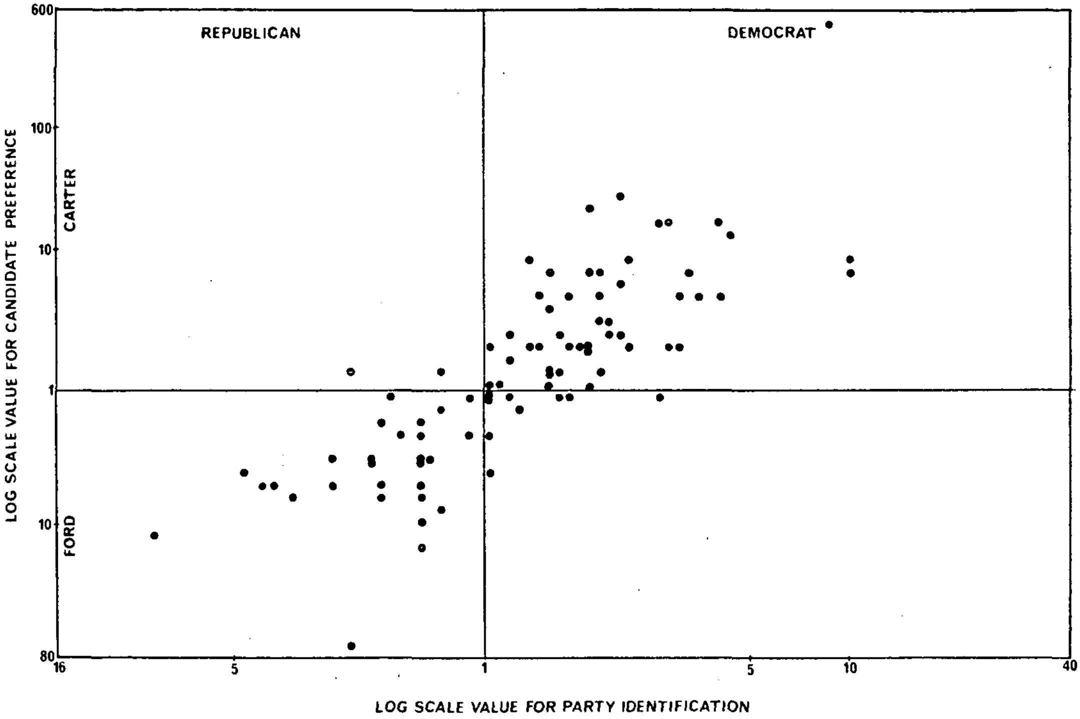
[10] At this stage in the testing of scale types, our estimate of relative costs is that category scaling costs information, magnitude scaling costs time. Assuming both category and magnitude scaling are included in a survey instrument (to maintain comparability with past items, directly compare measures, and evaluate their competitive power in testing hypotheses) and assuming two magnitude measures are calibrated and used in the cross-modality matching paradigm, the addition of magnitude scaling in survey research costs in time: 6–7 minutes per respondent for the calibration of number and line responses, 5–6 minutes of instruction and practice per respondent in the use of line and number to express strength of political opinion, and 15–20 seconds for the typical respondent to make line and number responses per question. A full-fledged cost-benefit analysis must await more extensive comparisons between measures in terms of the development of quantitative theory and testing of hypotheses.

[11] A detailed step-by-step manual for the magnitude scaling of political opinion in survey instruments is available from the Laboratory for Behavioral Research, Department of Political Science, SUNY, Stony Brook, Stony Brook, New York 11794.

investigators to record judgments as precise as possible: if an individual is capable of distinguishing between two items, that information can be conveyed through magnitude scaling. Finally, and this is critical in social scaling, the cross-modality matching paradigm makes it possible to validate psychophysically a respondent's magnitude judgments.

Essentially, category scaling distorts the expression of political opinion in systematic and predictable, but uncontrollable, ways. When the range of social opinion is greater than the category scale can measure, as is typically the case, or when the range of opinion is less (Baird, Kreindler, and Jones, 1971), there is a distortion of responses: the greater the discrepancy between true and artifactually imposed range, the greater the distortion. The solution, however, does not lie in an arbitrary increase in the number of categories in the category scale (because the true stimulus range is not known beforehand and varies from individual to individual), but rather in avoiding investigator-imposed constraints on responses.

As these comparative data indicate, when a small range of categories is provided for expressing strong opinions, most of the distortion occurs in the end categories—the overall effect being to vitiate the expression of strong opinions. This is particularly serious because many theories of behavior posit a relationship between strength of opinion and the likelihood of a congruent behavior. That is, those expressing the strongest beliefs and preferences are most likely to behave in accord with their opinion, while those less strongly engaged are less likely to act on behalf of their opinions. Because of the wide range of opinion in the endmost categories, a researcher cannot be confident whether a respondent choosing a polar category is expressing a moderate or intense opinion. As a result, attempts to predict subsequent be-



Source: Data collected from suburban New York sample by the Laboratory for Behavioral Research, SUNY, Stony Brook, October 1976 survey.

Both variables are measured psychophysically and the relationship is plotted in logarithmic coordinates. The slope is 2.0 and the correlation is .81.

Figure 9. Candidate Preference for Ford vs. Carter (Y-Axis) as a Function of Party Identification (X-Axis) for the October Sample.

havior as a function of a categorical expression of strength of opinion are seriously jeopardized.

A major consequence of weak measurement is weak theory. Given ordinal-level data, one is unable to test quantitative hypotheses rigorously. Thus, one can state only that an increase in *X* is likely to accompany an increase in *Y*, but the quantity of change in either variable cannot be specified. For this reason Tufte (1969) and others (Heise, 1975; Namboodiri, Carter, and Blalock, 1975) emphasize the utility of regression analysis, often in conjunction with correlational analyses. Empirical results such as ours, however, as well as theoretical considerations and analyses of the scaling of physical stimuli, indicate that category scales do not produce regression coefficients which can be meaningfully interpreted. Since regression analysis is one of the most powerful tools available to contemporary political scientists, further comment on this problem is in order.

Regression coefficients are arbitrary when produced by variables measured categorically because of the arbitrariness of the categorical measure—first, in the number of categories imposed by the format of the scale, and secondly, in the numbers which are assigned to the categories. Essentially, different sets of arbitrarily assigned, intervally spaced values produce different regression coefficients. More specifically, where the stimulus range is greater than the response range, the slope will be lower than it should be, and where the stimulus range is less than the response range, the slope will be steeper than it should be—solely as a result of the arbitrary constraints imposed by the format of the scale. Because conventional category scales typically provide a far smaller response range than the opinion ranges which they purport to measure, and because magnitude measures track the stimulus range relatively well as a result of the lack of constraints upon the respondent, one routinely finds that the regression coefficient for category scales numbered in the conventional fashion is lower than that obtained with magnitude measures (Curtis, 1970; Ward, 1972, 1973; Marks, 1974).

Tufte's (1969) argument, alluded to earlier, is that the ordinary assignment of numbers to categories is disadvantageous as well as theoretically and empirically untenable. However, his solution to the problem has two major drawbacks. Tufte suggests that investigators assign numbers to the categories which use their knowledge about the meaning of the response to reflect more accurately the true relationships between the responses. The first problem, of course, is the arbitrariness of the method, which depends upon some consensus among researchers as to the meaning of responses and agreement on the numbers assigned to the categories. The second problem is that the movement toward 7-point category scales, with only the endpoints labeled, makes it difficult to do other than assume that the categories are equally spaced—an empirically untenable assumption.

On the basis of the comparisons between scale types presented here, we conclude that category and magnitude scales measure judgments differently. In measuring strength of political opinion—as with the perceived intensity of physical variables—category scales appear to measure the perceived similarity-dissimilarity of stimuli, while magnitude scales measure the relative quantity of stimuli. In practice, then, where a researcher requires simple difference judgments, category scales suffice. Where, on the other hand, researchers propose to test quantitative hypotheses, magnitude scaling is a more appropriate method.

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Type-Set Politics:
Impact of Newspapers on Public Confidence*

ARTHUR H. MILLER
EDIE N. GOLDENBERG
LUTZ ERBRING
University of Michigan

This study combines survey data from the 1974 American National Election Study with the front-page content of 94 newspapers in an investigation of the relationship between the degree of negative political criticism found in newspapers and their readers' feelings of trust in government and a sense of their own political effectiveness. Although newspaper reporting was primarily neutral or positive, readers of highly critical papers were more distrustful of government; but the impact of criticism on the more stable attitude of political efficacy was modest. Level of exposure to national news interacted with critical news content primarily to affect feelings of trust, and not efficacy.

This article posits a structural explanation of inefficacy as a result of accumulating distrust, where policy dissatisfaction, rather than dislike of incumbent leaders, acts as the main determinant of cynicism. In this model, media criticism serves as a "mediator" of political realities which eventually, although indirectly, affects political malaise.

The past decade has witnessed substantial declines in both the public's sense of political efficacy and their confidence in government, shifts in attitude that occurred to a great extent prior to Watergate and the other scandals of the Nixon administration. This trend of declining support for the political system has been widely discussed and well documented (Converse, 1972; Miller, 1974) but explanations for it differ. Among the various causes that have been suggested are: dissatisfaction with the government's performance in various policy areas (Aberbach and Walker, 1970; Miller, 1974); partisan appeals of particular incumbents (Citrin, 1974); the social turmoil created by the civil rights movement and the Vietnam war (Abramson, 1972; Converse, 1972); and anti-government coverage in the media (Robinson, 1976; McLeod et al., 1977).

The development of a more general explanation of system support has been deterred, however, by several shortcomings of these studies. Seldom has more than one explanation been considered and tested in the same analysis; rarely have both concepts—efficacy and trust—been studied together in an effort to highlight their similarities and differences; and even more infrequently have direct measures of the presumed causes of the decline in system support been available for analysis. Previous studies have relied instead on measures of individual perceptions and self-reported behavior.

The concepts of political trust and efficacy, frequently treated as two dimensions of political alienation, will be examined below. *Political trust* will refer to the public's basic evaluative orientation toward the government in Washington. It will be measured as a dimension that runs from high trust or confidence in government to distrust or political cynicism. Cynicism thus indicates a negative evaluation of government and reflects the belief that the government is not functioning in accordance with individual expectations of efficiency, honesty, competence and equity. A sense of *political efficacy*, on the other hand, denotes the feeling that an individual and the public can have an impact on the political process because government institutions will respond to their needs. The lack of efficacy or the feeling of inefficacy indicates the belief that the public cannot influence political outcomes because government leaders and institutions are unresponsive to their needs. (See Appendix A for the survey questions used to measure these attitudes.)

The analysis reported here is primarily focused on the unique contribution of media content to political malaise and employs a direct measure of that content. By tracing the impact of degree of negative political criticism in newspapers on the political trust and, ulti-

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mately, on the political efficacy of the readers of these papers, we attempt to move closer to the presumed real-world causes of attitudes toward government. At the same time, we learn more about the role of mass media in attitude formation. Having analyzed both efficacy and trust, we can draw conclusions about the theoretical similarities and differences between these concepts and their interconnection. And by simultaneously examining the impact of the media and other competing explanations for inefficacy and distrust, we can also briefly explore the implications of our findings for general theories of alienation from government.

Given that we have content data from newspapers alone, it is not within the scope of this study to yield more than indirect evidence of the impact of television news reporting. Nevertheless, it is clear that if differing news reporting practices affect the fundamental orientations of the public toward its political leaders and institutions, the role of the news media—whether the print media, ratio or television—is an important area of investigation.

Linkage Analysis (or, Not All Media Content is Alike)

Does the reporting of news in a critical or negative style by the media taint the audiences' confidence in government and their feelings about the political system in general? Earlier studies have argued that media effects on attitudes are in fact minimal (summarized in Klapper, 1960 and de Sola Pool et al., 1973). More recently, others have reported substantial relationships between various media measures and political attitudes (Robinson, 1976; McLeod, et al., 1977). Why such contradictory findings? "Directional," evaluative media research has been confounded by the frequent use of, at best, indirect measures of media impact which require large inferential leaps. It has suffered from the methodological problems inherent in a failure to distinguish between reliance on a medium and exposure to a message, or between exposure in general and exposure to particular message content. Furthermore, recent trends in media research have deemphasized persuasion or influence effects, focusing instead on information and cognition functions of media use.

Previous analysis of media impact on political attitudes generally has presented either of two types of evidence in support of its claims: relationships between media *exposure* (or reliance) and political attitudes or case-study material on media *content*. As an example of the first, Robinson (1976) reported with survey data from the sixties that those who relied on television more than on other media sources were less politically efficacious than those who relied on newspapers, radio or magazines for their political news. From these data, plus results of an experiment involving the documentary, "The Selling of the Pentagon," Robinson generated his theory of videomalaise.[1] But no analysis of the relationship between media exposure (or reliance) and political attitudes can really answer questions about media impact on political efficacy or trust, because such an analysis can only assume, without empirical evidence, that there is something about the media content to which people have been exposed that affects individual attitudes. While Robinson argues that the content and style of television news in the 1960s was anti-institutional and negative, he has no hard evidence to support this assumption. The measurement of media content is essential if one wishes to demonstrate conclusively that political evaluations may become more negative as a direct consequence of news reporting that is critical of political institutions.

On the other hand, case studies of media content are not adequate, either. Paletz, Reichert and McIntyre (1971) found that the newspaper in Durham, North Carolina reported in a positive way on the activities of the Durham city council. Determining whether or not this coverage resulted in increasingly positive political attitudes, or in lower levels of interest, as the authors speculated, requires information on both individual attitudes and exposure to particular media messages—data they failed to gather.

Only an interconnected data set of survey responses and media content allows one to move beyond analysis based on measures of media exposure or media message alone to consider the actual media content to which people have been exposed. We produced such a data set by merging newspaper content data with survey data after matching respondents *with the particular paper they had actually read* (as determined from the interview).

As part of its study design, the 1974 American National Election Study provided both survey data for that year and a content analysis of all front-page articles (about 8900) for 94 newspapers collected from all the survey sample places across the country on ten noncontiguous days over the three weeks preceding the interview period (October 16, 17, 24, 25, 27, 29, 30, and November 1, 3, and 4). Each newspaper that was content-coded had a minimum of 7 survey respondents and an average of 16 respondents who read the paper on a daily basis.[2] Given this combined survey and media content data, the degree of political criticism within a newspaper could be correlated, at the individual level, with the attitudinal measures of efficacy and trust in government.

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[1] We have elsewhere (Miller, Erbring and Goldenberg, 1976) reported on our reexamination and reinterpretation of these data using a more valid measure of exposure to television national news, where our results run contrary to those of Robinson's.

Measuring Media Criticism. We measured the concept of institutional criticism by coding the content of each newspaper article to indicate direction of evaluation, as well as the object under evaluation. Coders were instructed to read an entire article for words indicative of negative criticism (such as *wrong, incorrect, harmful, misguided,* etc.) or praise (*helpful, intelligent, effective,* etc.) that were aimed at a specific person, policy or action. Care was taken to insure the coding of criticism on the basis of the manner in which the news was presented rather than on the subject of the story itself (e.g., a Watergate revelation). Each article was then coded as containing either criticism, praise, both or neither; the object of the criticism or praise was likewise coded, thereby identifying the political leader, institution or policy that had been evaluated in the article.

Our indicator of degree of criticism in each paper was determined by the proportion of articles containing negative words either on the entire front page or among some subset of front-page articles. It is thus important to keep in mind that while we use different terms interchangeably throughout this discussion when referring to degree of criticism—sometimes speaking of critical newspapers, sometimes of critical content or critical reporting—phrases such as "degree of criticism" or "highly critical" do not refer to the intensity of criticism within particular articles nor to the extent to which the criticism may be attributed to the particular newspaper itself,[3] but to the *proportion of stories containing any adverse criticism at all.*

"Degree of criticism" as used here then refers to the place of political conflict and controversy in the reporting practices of the press. If politics is conflict, then criticism of actors and policies by political opponents is ubiquitous and inevitable. But the extent to which such criticism is echoed by the press may well vary systematically if some papers are more eager than others to open their pages to the tides of political controversy.

Distribution of Criticism. Generally, newspaper coverage was not particularly negative. Among front-page articles with political or public policy implications—which accounted for three-quarters of all front-page stories—few (6 percent) offered praise and some (31 percent) carried adverse criticism, but the majority (63 percent) contained neither. However, newspapers did vary significantly in the proportion of critical front-page articles they carried. Some published as few as one in ten, while others contained a majority of front-page articles with criticism.

Not only were certain papers more critical, but degree of criticism also varied depending on the particular topic covered (see Figure 1). For example, articles on unemployment and shortages consisted primarily of straightforward reporting of government-released figures, with little mention of policy debates, while other topics were clearly more controversial. Particularly high in criticism were stories about confidence in government (52 percent of all articles published in this area made some critical reference), Watergate, governmental power, and government competence.[4] Moreover, the degree of criticism found for articles in these four high-criticism topic areas was intercorrelated across newspapers (range of .2 to .5), indicating that those newspapers carrying a high degree of adverse criticism in one area had also published a high proportion of negative articles in the other three.

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[2] Newspapers with fewer than seven readers in the survey, accounting for 15 percent of the survey respondents, were excluded from the analysis because of the minimum readership rule. This criterion was necessary, however, to maintain statistical reliability.

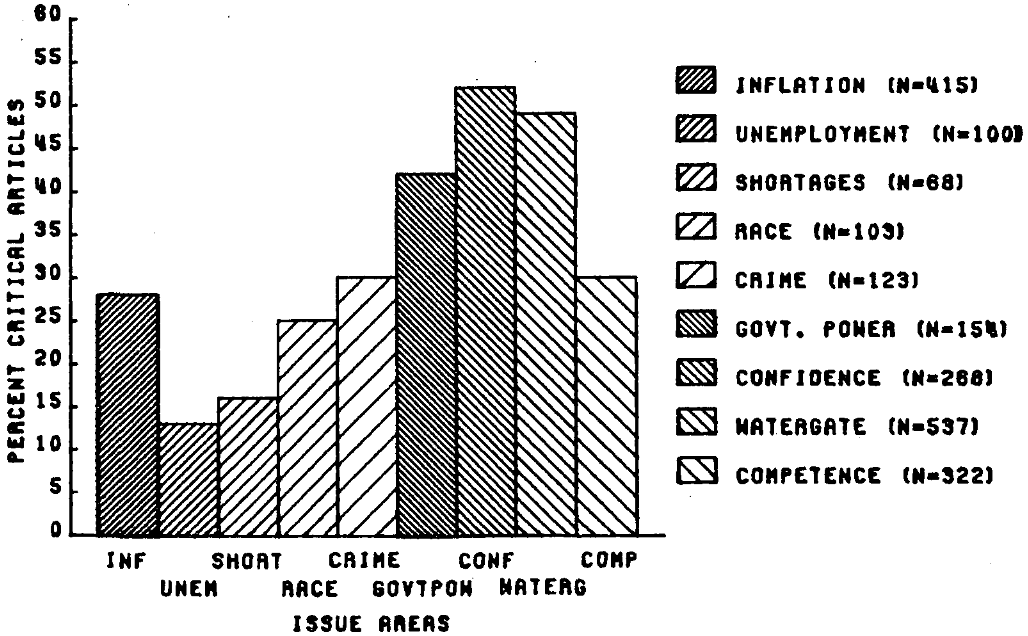
[3] As a point of information, the amount of criticism directly attributable to newspapers was obtained by cross-tabulating the public figures in the news with the object of criticism. In only 11 percent of the articles was the newspaper the source of criticism; in the remainder, criticism was attributed to an individual referred to in the article.

[4] As our analysis dwells on the subset of articles in these four domains, it is important to understand the types of stories included there. Articles on confidence in government were primarily concerned with allegations that government and politicians catered to special groups, charges of conflict of interest and dishonesty, as well as the broader topic of trust in political leaders and the system. Stories about Watergate focused on the defendants and prosecutors, as well as Ford's pardon of Nixon. Government power articles discussed government regulation of businesses; control of information, secrecy, and activities of such intelligence-gathering agencies as the CIA and FBI; and the debate between Congress and the president over

Because the four high-criticism issue areas were interrelated, as well as similar in content to the survey questions that have been used to measure trust in government (see Appendix A), we combined them into a single measure tapping evaluations of government more generally. This provided us with a more reliable[5] set of articles from which to measure newspaper criticism of government. With governmental evaluation so defined, each newspaper carried an average of 14 such stories over the ten sample days, which made up 15 percent of all front-page stories and 20 percent of all articles having explicit political implications.

While newspapers did not vary a great deal in the amount of coverage of these topics, there were large differences in how the material was presented to the public. A distribution of all newspapers according to the degree of criticism in articles on governmental evaluation reveals that for one-quarter of the newspapers, these stories were relatively free of inherent controversy and criticism (more than 60 percent of articles evaluating government were criticism-free), for another quarter, the news coverage was predominantly negative (over 70 percent of the articles contained some critical reference); and among the remaining half, the proportion of articles containing criticism ranged anywhere from 40 to 70 percent.

There were also substantial differences across newspapers in the proportion of government evaluation articles containing criticism. In the *Bridgeport Post* and the *Greenville Daily Reflector,* for example, only 18 and 23 percent, respectively, of the articles evaluating govern-



Source: Center for Political Studies Media Content Analysis Study, 1974; available through the University of Michigan, ICPSR. Not to be cited without full bibliographical reference to the present article.

Figure 1. Degree of Criticism by Issue Area

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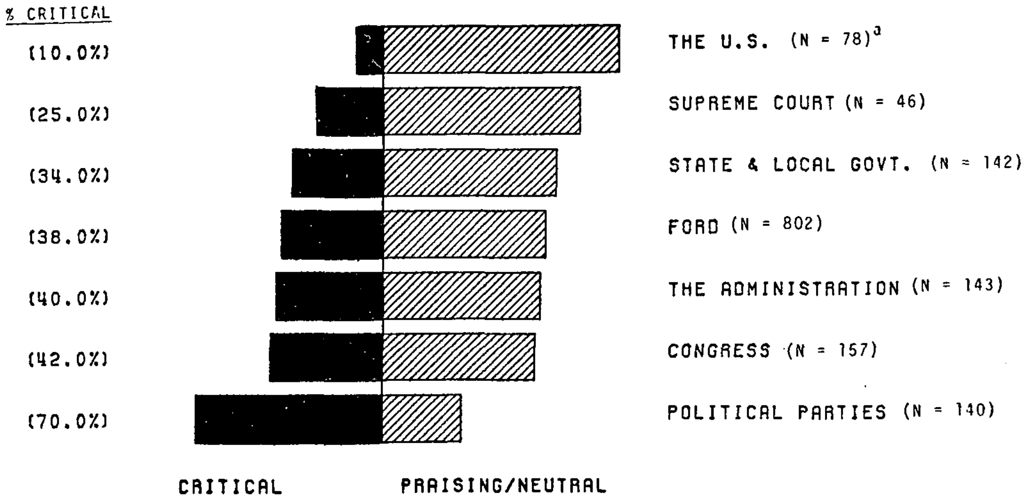
presidential power. Articles treating government competence included stories about the quality of public employees and the civil service, increasing costs of government, and the level of compensation for government employees, politicians and bureaucrats.

[5] The correlations between degree of criticism in each topic area and the combined government evaluation measure ranged from .67 to .73. The validity of the combined measure was supported by a high correlation (.66) with another index measuring criticism directed at political leaders representing the federal government, and by lower correlations (average

ment performance were critical; comparable figures for the *New York Times* and the *Washington Post* are 62 and 71 percent. Despite the latter figures, big-city[6] newspapers generally contained somewhat fewer critical stories on evaluations of government (52 percent) than did rural papers (60 percent). This is somewhat surprising, given the commonly accepted assumption that small-town newspapers are uncritical consensus-builders while large-city dailies generally report conflict (Donohue et al., 1972). Of course, the topic areas do not reflect specifically on small towns themselves but rather on the performance of the government in Washington. Still that does not account for the higher amounts of criticism in small-town newspapers than in the big-city dailies.

Targets of Criticism. Who bore the brunt of all the newspaper criticism? A variety of politicians, political and social institutions and public figures were favorite targets, although some were more frequently attacked than others. Few front-page articles about the "United States" were critical of it (10 percent), while an overwhelming majority of articles about political parties criticized party organizations (see Figure 2). Although we do not have data from which to make a causal argument, it is interesting to note the high degree of criticism directed at political parties at the same time that they were held in such low public esteem. Perhaps the recent declines in both party identification and party loyalty were, at least in part, reinforced by the negative image of political parties as conveyed in the mass media.

While newspaper coverage was also critical of Congress, the federal administration and Ford, much of it grew out of confrontations between the executive and legislative branches of the federal government. After Watergate, the Democrat-controlled Congress, in attempting to redress the imbalance of power between itself and the presidency by actively pursuing a legislative program of its own making, came to loggerheads with President Ford. Determined to keep Congress in check, Ford often resorted to the veto or public criticism as a means of attaining his goal. In 66 percent of the articles in which Congress is the object of criticism and in 81 percent of those targeting the Democratic party, the source of the criticism can be traced to Ford. Similarly, 25 percent of the criticism



Source: Center for Political Studies Media Content Analysis Study, 1974; available through the University of Michigan, ICPSR. Not to be cited without full bibliographical reference to the present article.

[a] The number of articles about a political object that were available for computing the percentage of critical articles.

Figure 2. Degree of Criticism in Articles about Selected Political Objects

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relationship of .19) with indices of criticism in the topic area of economics.

[6] The urban-rural variable used here ranged from the 12 largest cities through smaller cities to suburbs and outlying areas and thus reflects city size more than geographic area.

of Ford that appeared on the front pages of newspapers originated in Congress.

Turning to articles dealing exclusively with evaluations of government, we find a considerably different distribution of targets of criticism. Here, either Nixon or the Watergate defendants and prosecutors were by far most frequently criticized (40 percent), although government and politicians in general also came under attack, with the president, vice-president and other administration leaders as targets of criticism in 18 percent of the government evaluation articles and Congress accounting for another 9 percent. State and local government received a good deal of attention as well, appearing as the target of criticism in 19 percent of these articles.

It should be noted that while we have not distinguished between individuals and institutions appearing in the news, criticism in the newspapers was usually directed at particular political leaders and incumbents, rarely at the office or the system per se. For example, criticism of Ford appeared quite frequently in the papers, but *criticism of the presidency was virtually nonexistent.* Congress as an institution did come under attack, however, in nearly half of the articles criticizing senators, U.S. representatives and "Congress." But, as noted earlier, the criticism of Congress was almost entirely attributable to President Ford. The general category of state and local government, on the other hand, chiefly included references to governors and other state and local officials, while the institutions of "state and local government" were rarely mentioned.

In summary, front-page newspaper coverage of events with political relevance was predominantly neutral: most articles could not be coded as involving either praise or criticism of political actors or institutions. Newspapers did vary greatly, however, in the proportion of front-page articles containing criticism. Some papers carried relatively few stories with criticism; others relatively many. The proportion of critical articles also varied by subject. A much larger proportion of adversely critical articles was found for stories about competence, efficiency and integrity of government—subjects belonging to the general domain of trust in government in public opinion studies—than for stories on other topics such as inflation or unemployment. Criticism was directed primarily at the federal government and in particular at past and incumbent representatives of the federal administration. The administration was not, however, the only target of criticism; Congress, state and local government, and political parties were also criticized.

Explaining Discontent with Content

Newspaper stories dealing with honesty, integrity and competence in government were not only critical but also both prevalent and prominent[7] in the months prior to the 1974 election. It seems reasonable, therefore, to hypothesize that media coverage of these events would promote a sense of political disaffection, that is, feelings of powerlessness and cynicism among the populace. Learning that certain political incumbents have behaved dishonestly, particularly if they have just been put into office by an overwhelming electoral mandate, could be expected to create political disaffection.

We are not merely suggesting that after people learned about the corruption in their government through the "objective" unfolding of political reality in the news, they then proceeded to draw their own subjective conclusions about these events and eventually became disillusioned with politics. Rather, we are asking the more crucial but less obvious question of whether or not variation across newspapers in the presentation of media content—that is, the degree to which the content was critical of political incumbents and institutions—also had an independent impact on attitudes such as political trust and efficacy.

We expect to find that the unique impact of the media goes beyond the cognitive process to affect directly the reader's evaluative orientation toward politics. Given the aforementioned primary focus on the incumbent and the individual, rather than the office or institution, as a target of criticism, we also expect that media criticism may have a greater impact on trust than on efficacy. Citrin (1974) and Muller (1977) have argued that trust in government really represents evaluations of political incumbents; and according to Converse (1972) and Balch (1974), our political efficacy index is a measure of institutional responsiveness. Other research also raises this hypothesis by confirming that efficacy is the more stable of the two attitudes (Converse, 1972; Miller, 1974) and hence may be more resistant to media impact.

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[7] Prominence was measured by the size of the headlines awarded a particular story. The actual point size of type used by each newspaper was measured and scaled so that a trichotomous coding (small, medium, large) for a headline was relative to each individual paper. On the average, articles negatively evaluating government had somewhat larger headlines (71 percent had medium and 16 percent had large) than did neutral or favorable stories (54 percent had medium and 17 percent had large).

The Impact of Critical Content. As the results presented in Table 1 reveal, political disaffection was significantly related to negative newspaper criticism in 1974, even after one controls for education and media exposure; and the effects of newspaper criticism appear to be stronger for political trust than for political efficacy. Moreover, the degree of criticism in newspapers (critical content) seems to have had a somewhat greater impact on the attitudes of people educated only through grade school than it did on those with more education (compare the partial correlations and *b* coefficients across education levels).

Although the size of the regression coefficients (*b*) for the content variable appear small, they are quite significant. Their size is an artifact of the spread of values on that variable which ranges from 0 to 100 because negative content was measured as the percentage of newspaper articles having criticism. For example, the coefficient for critical content in the equation explaining political cynicism among those with only a grade school education (b = .012) indicates that there will be a .012 unit change in cynicism for every unit change in the content variable. Thus, we find that the difference in cynicism between a white grade school-educated person who reads a newspaper with, for instance, only 30 percent critical articles, and a similarly educated white person who reads a newspaper in which, say, 70 percent of the articles are critical is one-half unit[8] on the

Table 1. Regression Equations Predicting Cynicism and Inefficacy from Critical Content and Media Exposure—Whites Only by Levels of Education

	Predicting Cynicism[e]			Predicting Inefficacy[f]		
	Partial	*b* Coefficient	Significance	Partial	*b* Coefficient	Significance
Grade School (N = 128)						
Critical Content	.23	.012	.011	.15	.006	.100
Medium Exposure TV[a]	.14	.884	.142	–.21	–.849	.024
High Exposure TV[b]	.17	1.025	.057	–.11	–.374	.257
Medium Exposure Papers[c]	–.19	–.764	.039	–.16	–.402	.083
High Exposure Papers[d]	.08	.360	.404	–.13	–.383	.154
	Constant = 2.45		R = .40	Constant = 1.42		R = .29
High School (N = 583)						
Critical Content	.11	.006	.009	.10	.005	.041
Medium Exposure TV[a]	.10	.605	.012	.00	.001	.996
High Exposure TV[b]	.17	.826	.000	.02	.079	.555
Medium Exposure Papers[c]	–.14	–.638	.001	–.21	–.644	.000
High Exposure Papers[d]	–.10	–.504	.012	–.21	–.683	.000
	Constant = 3.64		R = .21	Constant = 1.94		R = .25
College (N = 400)						
Critical Content	.13	.007	.011	.08	.004	.110
Medium Exposure TV[a]	–.05	–.288	.283	.06	.206	.265
High Exposure TV[b]	–.03	–.129	.548	.05	.190	.315
Medium Exposure Papers[c]	–.09	–.494	.110	–.22	–.972	.000
High Exposure Papers[d]	–.05	–.228	.301	–.19	–.854	.000
	Constant = 3.73		R = .18	Constant = 1.90		R = .24

Source: Center for Political Studies National Election Survey and Media Content Analysis Study, 1974; available through the University of Michigan, ICPSR. Not to be cited without full bibliographical reference to the present article.

[a] A dummy variable with those who watch the national evening TV news "sometimes" coded 1; all others are 0.

[b] Dummy variable; 1 = watch national evening TV news "frequently"; all others coded 0.

[c] Dummy variable; 1 = read about national politics in daily newspaper "sometimes"; all others are 0.

[d] Dummy variable; 1 = read about national politics in daily newspaper "frequently"; all others are 0.

[e] The dependent variable is a five-item scale ranging from 1 = trusting to 6 = cynical. *Positive* regression coefficients indicate that higher criticism in media content, or higher exposure, corresponds to higher levels of cynicism.

[f] Efficacy is a three-item scale; 1 = efficacy, 4 = inefficacious feeling. *Positive* regression coefficients indicate that higher criticism in media content, or higher exposure, correspond to higher levels of inefficacy.

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[8] The calculations from which this figure was derived are: 30 percent negative criticism x .012 = .36;

cynicism scale—a fairly significant difference.

We would like to reemphasize at this point that the content measure used in the analysis here is the degree of criticism in newspapers regarding the evaluation of government or the proportion of those articles containing any criticism. It is not a measure of the amount of coverage given to stories about dishonesty or corruption in government. Because amount of coverage varied from paper to paper substantially less than did criticism, adding a coverage amount variable to the regression analysis had the limited effect of increasing only slightly the overall proportion of variance in efficacy and trust that was explained by the regression equations, while leaving the other coefficients virtually unchanged. Apparently, by the end of 1974 the public had received, and was continuing to receive, so much information about Watergate and other government scandals that variation in newspaper coverage of these topics was minimally related to feelings of political disaffection, compared to the effects of degree of criticism as conveyed by each paper in its reporting of these events.

Another variable we initially expected to have some influence on the regression analysis results, but which did not, was the source of origin of the story—that is, whether it came from a wire service, some other newspaper service, or a local reporter. Although nearly four out of every ten articles originated with a news service, there were generally enough other articles on the relevant topics to provide sufficient variation for measuring criticism. Moreover, we sometimes found that a wire service story would be printed in a shortened version and edited differently by each newspaper in the process.

Beyond indicating a relationship between critical content and political evaluations, the regressions of Table 1 provide for a comparison[9] between media *exposure* (that is, the amount of exposure to national news) and media *content* (that is, the amount of criticism in government evaluation stories) as explanations of political disaffection. Television exposure was defined as how often respondents said they watched the early evening national news (*never, rarely, sometimes,* or *frequently*), and its distribution range was 10, 8, 22 and 60 percent, respectively, in 1974 for the total sample. Newspaper exposure was similarly defined as how frequently respondents said they read newspaper articles about national politics, and its total sample distribution was 33, 11, 30 and 26 percent, respectively. With dummy variables measuring medium ("sometimes") and high ("frequently") levels of both newspaper and television exposure included in the regression equations along with the critical content variable, we find that negative newspaper content has a direct effect on both trust and efficacy that is independent of, and in addition to, the effects of general exposure to public affairs, whether through television or newspaper.

Moreover, critical newspaper content is consistently and significantly related to both dependent variables across the levels of education. In each case, the higher the degree of newspaper criticism, the more inefficacious and distrusting were the readers. Presumably this result can be attributed to the impact of variation in newspaper content. Given the absence of choice in most media markets, it seems more reasonable to assume that the relationship between media content and political attitudes flows causally from the medium to the individual, rather than to argue that disaffected individuals seek out critical newspapers.

It is possible, of course, that newspapers tend to reflect the attitudes of their readers and the surrounding community when reporting the news (Sears, 1968; Pollock and Robinson, 1977; Pollock et al., 1978), an argument which suggests that causality would flow from the disaffected attitudes of the public to the newspaper's style of presentation. But this argument does not appear very plausible for the type of dependent variables investigated here. Not only are the subscribers to any particular newspaper heterogeneous, but also critical newspaper content is related to political disaffection even after controlling for factors such as partisanship or social characteristics that might reflect readers' attitudes. It seems relatively safe then to assume that political disaffection is partially caused by the critical tone of newspaper reporting of national political news.

Limitations of Exposure Measures. Exactly what the exposure measures are tapping is not, however, entirely clear. In stark contrast to the content variable, the relationships between the exposure measures and cynicism or inefficacy are both inconsistent and contrary to expecta-

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70 percent criticism by .012 = .84; with a resulting difference of .48 units on the dependent variable.

[9] Because the regression coefficients presented in Table 1 are unstandardized, the exposure the criticism *b*'s cannot be compared directly. However, the multiple partial correlations can serve as a gauge of the relative impact of content and exposure.

tions. Greater exposure to newspapers is, for example, positively related to higher feelings of efficacy for citizens at all educational levels. If we recall that roughly 70 percent of *all* front-page news was found to be neutral or positive, this seems reasonable. Of course, the same relationship would be expected if there were a tendency for more efficacious people to be more interested in national politics and to read more frequently about such things than do less efficacious individuals. Cynicism has a similar pattern of relationships for newspaper exposure: the more frequent readers tended to be more trusting. For television, higher levels of viewing were negatively related with inefficacy but positively related with cynicism among those respondents with only a grade school education. This is a distinct reversal, given that those who feel inefficacious are also generally more cynical (the correlation between cynicism and inefficacy is .35). For the college-educated, we found the obverse: television exposure was negatively related with cynicism and positively related with inefficacy. Upon further investigation, we concluded that these inconsistent relations should not be attributed to media effects but rather to demographic factors that are confoundingly associated with media usage and thereby spuriously reflected in analyses employing self-reported exposure measures.[10]

Unfortunately, cross-section data that are gathered at a single point in time, such as the survey data used for the regressions presented in Table 1, do not provide a means for conclusively sorting out the causal dynamics linking media exposure and political discontent. The causality could reasonably flow in either direction, given that the particular medium (newspapers or television) the individual uses as an information source and the frequency of use may simply reflect social characteristics. What the regression equations do suggest, however, is that self-reported exposure measures are not to be trusted as surrogates for media content when one is testing for the impact of the media on directional or evaluative dependent variables. Exposure measures may be good indicators of a general awareness of current events, but unless all the news is one-sided, exposure will not be systematically related to any measure of evaluative orientation or direction.

One possible exception occurs when a recent event produces a uniform directional shift in the population. In that case, the most frequent media users become informed of the event before less frequent users. Consequently, the frequent users will show a systematically different orientation in their attitudes, at least until the information becomes uniformly distributed throughout the population. Here, differences in the saliency of the event can be attributed to media exposure, but the differences in attitude *direction* should be attributed to awareness of the event, not to the media.

An impact of the news media on the evaluative component of attitudes can be argued only if the event is reported differently by the various media and if this difference in reporting is systematically related with variation around the general trend of attitude shift caused by the event. Studies that employ only exposure measures are limited since they cannot separate the impact of events from a real media effect. Clearly, knowledge of both the extent of general exposure and the nature of the content is needed when the dependent variable is directional. The exposure measures capture the cumulative effect of general trends while the content measures allow for sorting out the unique media effect.

Levels of Exposure as a Conditioner. Given the inconsistencies and spurious relationships between exposure measures and political attitudes, should exposure indicators be eliminated from further analysis of media effects? Not necessarily. While media exposure is conceptually limited as an exaplantion of directional dependent variables, it could conceivably act as a conditioning factor heightening media impact on the reader's political attitudes. Theoretically, negative newspaper content should have a greater impact on the political disaffection of frequent readers of national political news than on occasional readers. Indeed, the validity of the relationships reported in Table 1 between newspaper criticism and political evaluations would be questionable if media content did not have a greater impact on those more exposed to it.

A model for estimating the hypothesized interactive effects of media content and exposure should, however, still incorporate separate measures of newspaper and television exposure, as well as critical content, in order to determine whether either exposure or content also contribute independently toward an explanation of political disaffection. More formally, this model may be described as:

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[10] Another inconsistency involved with the exposure measures is that clearly they are not linearly related to the dependent variables. Employing the exposure measures in the regressions as ordinal variables rather than as dummy variables produces much lower relationships with the dependent variables. Studies that utilize exposure measures as independent variables should be particularly concerned about nonlinear relationships that could produce correlations very close to zero.

$$Y = \alpha_0 + \alpha_1 d_1 + \alpha_2 d_2 + \alpha_3 d_3 + \alpha_4 d_4 + \beta_1 \chi_1 + \beta_2 d_1 \chi_1 + \beta_3 d_2 \chi_1 + e$$

where Y is either efficacy or trust; d_1 and d_2 are the dummy variables from Table 1 indicating medium and high levels of newspaper exposure; d_3 and d_4 are the dummy variables for medium and high exposure to television national news; χ_1 is the degree of newspaper criticism; and the multiplicative terms $d_1\chi_1, d_2\chi_1$ represent the interaction of exposure and content. Substantively, the most important terms of the model are the derived coefficients β_1, β_2 and β_3, as these indicate the "impact" of newspaper content on trust and efficacy for different levels of newspaper exposure.[11]

A least squares estimation of our model makes evident that newspaper criticism did not influence political efficacy and trust in government identically. Critical content again had a more substantial impact on cynicism than on inefficacy, regardless of education or exposure level. The relationship between newspaper criticism and trust was, however, differently affected by both amount of exposure and partisan ideology.

As previously argued, political efficacy is not only the more stable attitude,[12] but it is also in large part an expression of beliefs about the responsiveness of government institutions and the political system (Easton and Dennis, 1967; Muller, 1970). And because most of the critical news during the Watergate period focused on how various incumbents and political institutions were performing rather than on the legitimacy or responsiveness of those institutions, it is theoretically meaningful to find greater media impact on trust in authorities than on a sense of efficacy.

A comparison of the impact coefficients plotted for the total population within education levels in Figure 3 confirms the considerably lower impact of media criticism on efficacy than on trust that was found in Table 1. There was furthermore a minimal increase in the amount of variance explained with the new set of equations (compare the R's in Appendix B with those in Table 1). And basically similar effects were found for Democrats and Republicans alike, suggesting that efficacy, unlike trust, is relatively independent of the influence of partisan loyalty.

There was, however, a significant increase in the overall explanation of political cynicism with the new set of equations, indicating a "better fitting" or more appropriate theoretical model when the interactions between critical content and media exposure are included. Because this increase in explanatory power is especially evident among those with only a grade school education, it would appear both that their attitudes are more sensitive to media differences (the impact coefficients are higher), and that media impact is more likely heightened for them by the interaction of exposure and negative content.

There was also a greater impact of newspaper criticism on cynicism for those who read national political news more frequently than for occasional readers. The impact coefficients for medium and high levels of exposure are considerably larger than those for low exposure, regardless of education.

But while exposure clearly increases the impact of newspaper criticism on political cynicism, the increase is by no means monotonic. Among the general populace, frequent newspaper readers consistently appear to be less affected than moderate readers, especially among the college-educated. As the partisan coefficients[13] indicate, however, this may be explained in large part by an incumbency bias. Observed separately, Democrats (strong and weak combined) follow the expected monotonic pattern of an increasing media impact on attitudes among those with higher levels of newspaper usage. But there is a discernible tendency toward consistently lower media impact on Republicans who were most frequently exposed to political news. Either because of partisan loyalty or selective perception, Republicans who read newspapers frequently were apparently less likely to translate negative stories about a Republican administration into distrust of government.

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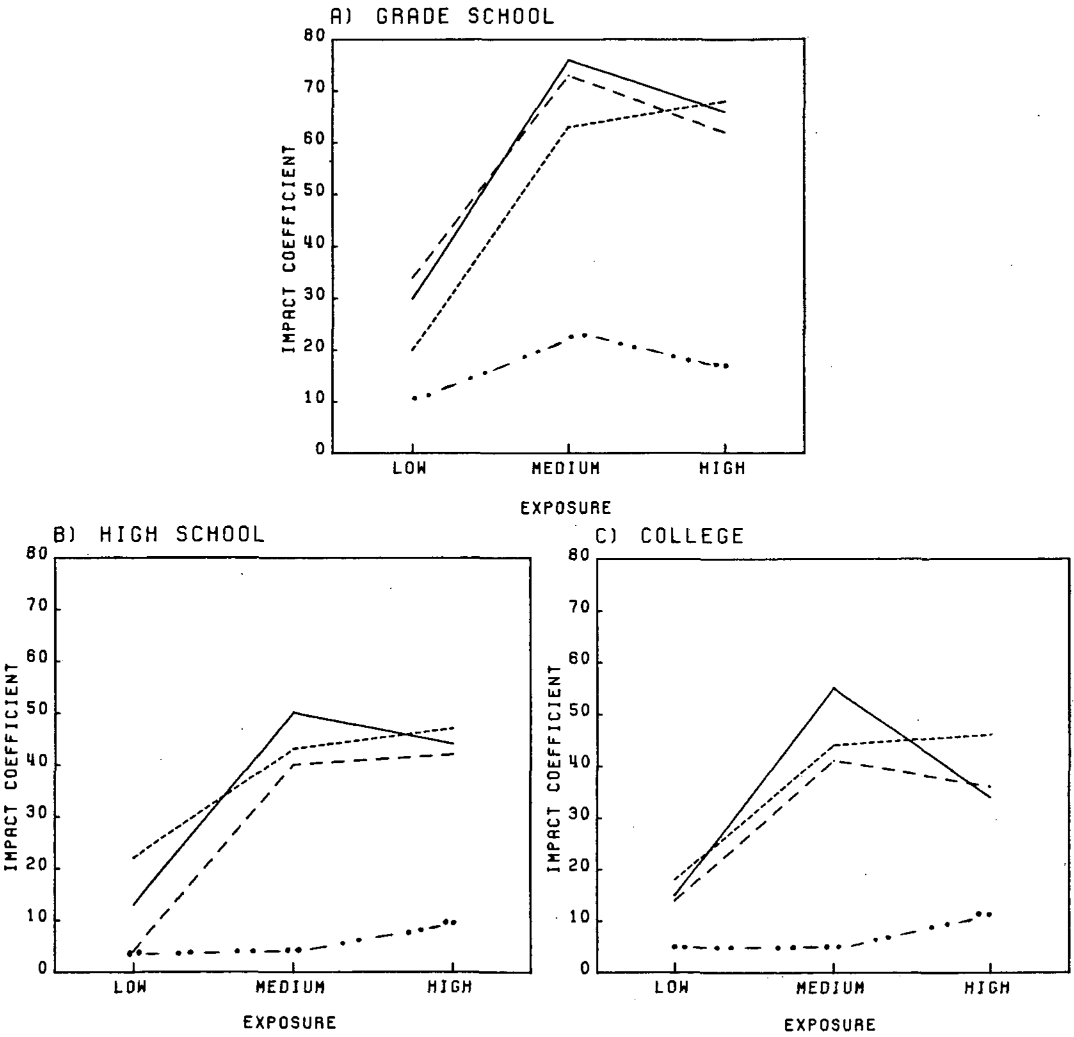
[11] The impact coefficients for the critical content variable were estimated from β_1 for those who "rarely" read about national politics in newspapers; from $\beta_1 + \beta_2$ for those who read the newspaper "sometimes"; and from $\beta_1 + \beta_3$ for "frequent" readers. Positive coefficient values indicate that as newspaper criticism increased by one unit, a sense of distrust or inefficacy also increased by a value equal to the impact coefficient times one unit on the criticism scale. See Appendix B for the complete equations.

[12] A comparison of the shift in the sense of efficacy and trust between 1972 and 1974 confirms the greater stability of feelings of efficacy. The decline in efficacy during those two years was only three percentage points, whereas trust declined by 15.

[13] Unfortunately, there were too few cases to compute separate equations for Independents reliably.

To recapitulate, the presentation of news in a manner that conveys a high degree of political conflict or criticism leads to a sense of distrust and inefficacy among newspaper readers. As hypothesized, newspaper criticism had more effect on trust than on a sense of efficacy, a result which supports the view that critical news was primarily directed at government performance rather than at the legitimacy of institutions. Exposure items were not consistently related to government evaluations, thus confirming our earlier argument that they are not reliable measures of media impact for explaining directional attitudes. However, exposure does condition responses to media criti-



Source: Center for Political Studies National Election Survey and Media Content Analysis Study, 1974; available through the University of Michigan, ICPSR. Not to be cited without full bibliographical reference to the present article.

[a]The scale for the impact coefficients must be multipled by .001 to equal the unstandardized regression coefficient. See Appendix B for the complete equations.

Figure 3. Interactive Impact of Newspaper Criticism and Exposure Level on Political Cynicism and Inefficacy[a]

cism, with frequent readers being more influenced by newspaper reporting than occasional readers.

Exploring Alternative Explanations

At this point in the analysis, we have demonstrated a significant relationship between negatively critical media content and evaluations of government. Nonetheless, the careful reader, in noting that the criticism in newspapers may simply have been a reflection of the existing general dissatisfaction with the Nixon/Ford Administration, might object that we are overstating our case. To establish conclusively that the treatment of the news had an additional and independent effect on political disaffection, we must therefore take into account alternative explanations for the pervasiveness of cynicism and inefficacy in U.S. society today.

Incumbency versus Policy Dissatisfaction. The current literature reports two main trends of thought. One regards disaffection from government, particularly as it is measured by trust, as a function of negative evaluations of the particular incumbent administration (Citrin, 1974; Muller and Jukam, 1977). If the public disapproves of the political values, partisan orientation or personal characteristics of an incumbent, they are likely to be less trusting of government. The other trend suggests that an accumulation of dissatisfaction with the policies pursued by a series of incumbents (hereafter referred to as *policy dissatisfaction*) results in the deterioration of trust (Miller, 1974) and efficacy (House and Mason, 1975). Thus, both disapproval of particular incumbents and dissatisfaction with governmental policies would contribute independently to negative evaluations of government.

An analysis incorporating evaluations of the incumbent and policy dissatisfaction as additional variables provides a test of the independent explanatory power of the media measures. It should be noted that we employed here the simple model underlying the analysis of Table 1, rather than the interactive model of Figure 3, so that the effect of media criticism as a single variable could be more parsimoniously compared with the importance of affect toward the incumbent and policy dissatisfaction. But in doing so, we slightly underestimated the relative importance of critical media content, as well as the overall explanatory power (R squared) of the equation.

The results of the regression analyses clearly reveal a significant effect of media criticism on both cynicism and inefficacy that is independent of either policy dissatisfaction or support for the incumbent (see Table 2). In predicting cynicism, the effect of media criticism is nearly

Table 2. Regression Equations Predicting Cynicism and Inefficacy from Media Content and Exposure, Policy Dissatisfaction and Incumbent Support–Whites Only[a]

	Predicting Cynicism			Predicting Inefficacy		
Predictors	Grade School	High School	College	Grade School	High School	College
Critical Content	.31**	.17**	.15*	.18*	.12**	.10
Medium Exposure TV	.12	.08	–.06	–.22*	.01	.09
High Exposure TV	.13*	.10*	–.03	–.16*	.01	.03
Medium Exposure Papers	–.12	–.09	–.10	–.16*	–.17**	–.17**
High Exposure Papers	.04	–.06	–.07	–.14*	–.19**	–.14*
Policy Dissatisfaction[b]	.39**	.25**	.23**	.17*	.15**	.12*
Incumbent Support[c]	–.16*	–.12**	–.08	–.06	.00	.00
R	.61	.41	.34	.32	.28	.25

Source: Center for Political Studies National Election Survey and Media Content Analysis Study, 1974; available through the University of Michigan, ICPSR. Not to be cited without full bibliographical reference to the present article.

[a] Table entries are standardized regression coefficients for comparing the impact of different predictors, and the multiple R for each equation. Significance levels indicated for the coefficients are: * = $p < .01$; ** = $p < .001$.

[b] Composite index incorporating appraisal of how well the government was doing in handling the "most important" national problem; how good a job it was doing in fighting inflation and unemployment; as well as the distance from the respondent to Ford and Nixon on the issues of government guaranteed jobs, urban unrest, and aid to minorities. Index values ranged from 1 = satisfied to 7 = dissatisfied.

[c] Rating of President Gerald Ford using a thermometer measure where 0 = maximum dislike, 50 = neutral, and 100 = maximum favorable rating.

as strong as that of policy dissatisfaction while the contribution of incumbent support is noticeably weaker. When the sense of inefficacy is the dependent variable, essentially the same pattern emerges, but at a substantially lower level.

Among the measures of exposure to the news media, only television exposure is consistently, if weakly, related with cynicism; those who watched television news more frequently were the most cynical. We are inclined to interpret this relationship at face value; presumably television reporting, in 1974, did much to highlight the unfolding of the aftermath of Watergate and other critical revelations obviously not conducive to fostering trust in government. By contrast, only newspaper exposure is consistently, and strongly, related to a sense of political effectiveness; those who read the papers most frequently scored *highest* on efficacy. Here again, however, we have the cause-and-effect question of whether feeling politically efficacious leads one to read the newspaper more frequently, or vice versa. We assume that this relationship reflects the impact of background socialization factors other than education which presumably sustain beliefs about political institutions or regime norms (Easton and Dennis, 1967), but which have less influence on the attitude of trust because it is based more on experience. Interestingly, for the least educated respondents, a similar effect on the feeling of efficacy is associated even with television exposure.

Relative to support for the incumbent, then, policy dissatisfaction is a considerably more potent explanation of both political cynicism and inefficacy. Positive evaluations of incumbent President Gerald Ford were indeed related with trust, but not as substantially as has been suggested. With the effects of incumbency controlled in the regressions, we can now assume that policy dissatisfaction taps a more enduring sense of discontent directed at government performance generally, implying a greater lack of diffuse support than would be suggested by mere disapproval of specific incumbents (Easton, 1975). In addition, of course, the impact of criticism in the newspapers on negative political evaluations remains substantial even after one controls for both policy dissatisfaction and evaluations of the incumbent administration. More importantly, the analysis results thus far tend to support the distinction we have made between distrust as indicating discontent with the performance of authorities and inefficacy as revealing a critical questioning of institutional or regime norms.

Toward a Model of Mediated Discontent. Trust and efficacy have in the past been treated as two theoretically separate dimensions of political alienation (Gamson, 1968; Finifter, 1970). Our analyses suggest a conceptual model that posits a structural explanation of inefficacy as a result of accumulating distrust. As discontent with government accumulates over time, we would expect, eventually, an erosion of citizens' beliefs about political institutions. The belief that institutions are not responsive to the needs of the people may, therefore, develop directly out of a pervasive sense of distrust of authorities; it would be influenced only indirectly by dissatisfaction with the incumbent administration and government policies or media criticism.

A model treating distrust as a principal cause of feelings of inefficacy fits the 1974 data remarkably well. Figure 4 presents the corresponding causal diagram and the statistically significant path coefficients estimated from the structural equations.[14] The model demonstrates that a sense of inefficacy is directly influenced by two main sources: a generalized attitude of accumulated discontent toward the authorities represented by political cynicism, and socialization or background factors that are apparently tapped by media exposure habits.[15] None of the remaining variables had a significant direct effect on feelings of inefficacy.

While efficacy and trust may be conceptualized as independent dimensions of alienation, they are strongly related, at least under the circumstances of 1974. Cynicism evidently reflects general dissatisfaction with government performance and not simply a lack of support for specific incumbents[16] and is at the same time a direct cause of disillusionment with broader regime norms regarding institutional evaluations. It may therefore be considered a "leading" indicator of diffuse support. Thus, research which posits policy dissatisfaction as a direct cause of inefficacy without taking cynicism into account (for example, House and Mason, 1975) commits a serious misspecification.

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[14] For the sake of brevity, we have presented the results from the total white subsample uncontrolled for education or party identification. Adding these controls does not significantly alter the results.

[15] The negative coefficient for the relationship between newspaper exposure and the feeling of inefficacy indicates that frequent newspaper readers felt more efficacious than occasional paper readers.

[16] Yet further evidence that trust in government does not simply reflect specific support of the incumbent authorities is offered by the fact that trust did not increase after Ford replaced Nixon (Miller, Brudney, and Joftis, 1975).

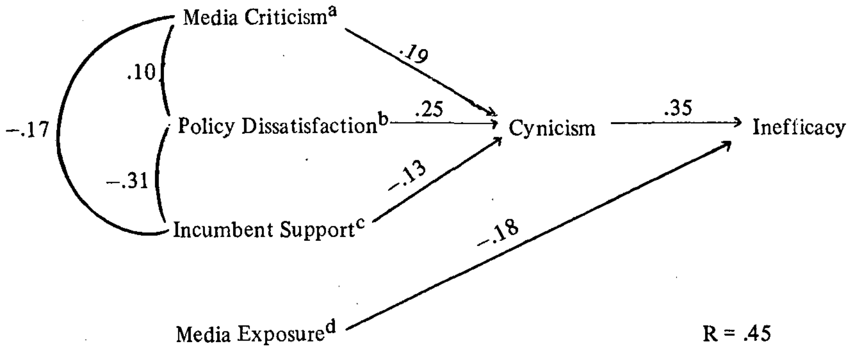
Our model clarifies the role of the media in the developmental process underlying citizen evaluations of government. Newspaper criticism was more strongly associated with ratings of Gerald Ford than it was with policy dissatisfaction. It is understandable that, at any given time, critical media content would have a stronger relationship with evaluations of incumbent authorities than with general assessments of policy performance, given that the target of media criticism was more frequently individual politicians than policies or institutions. We should expect this relative difference if dissatisfaction with government policies in various domains represents an *accumulating* negative evaluation of government performance over time.

Media criticism furthermore appears to lead citizens to question whether their government is operating according to expectations, but it does not directly influence regime norms. News reporting that emphasized political conflict, and thus exerted a significant influence on distrust of government, only subsequently and indirectly contributed to feelings of political inefficacy.

Conclusion

By the end of 1974, a majority of the adult population in the United States could be classified as politically disaffected: they distrusted government and they viewed its institutions as unresponsive to their needs. These judgments resulted both from the political scandals that had marred the second Nixon administration and from an evaluation of the performance of their political leaders and government when confronted with the social turbulence of the past decade. Their government's response to such challenges as the civil rights movement, urban riots, the Vietnam war, and the worst recession since World War II was no doubt the primary cause of continuing disaffection for most people.

As our research demonstrates, media style in reporting these events also had an effect on the degree of popular political disaffection found in America in 1974. Readers of papers containing a higher degree of negative criticism directed at politicians and political institutions were more distrustful of government, and also somewhat more likely to believe that the government was unresponsive, than were readers of newspapers containing less criticism. Moreover, amount of exposure to news content acted as a conditioner with an interesting partisan twist. Higher



Source: Center for Political Studies National Election Survey and Media Content Analysis Study, 1974; available through the University of Michigan, ICPSR. Not to be cited without full bibliographical reference to the present article.

[a] Degree of critical content in newspaper articles from the government evaluation domain.

[b] Composite index. See note b in Table 2.

[c] Thermometer rating of Gerald Ford.

[d] Frequency of exposure to newspapers (never, rarely, sometimes, or frequently).

Figure 4. Causal Diagram Depicting Role of the Media in Explanations of Cynicism and Inefficacy—Whites Only

levels of exposure interacted with adversely critical news content to yield higher levels of cynicism and feelings of inefficacy, but with differing patterns for Democrats and Republicans.

Overall, the full potential of a negative style of news reporting was largely unrealized in 1974; most newspapers were not adversely critical, and those that were, did not directly foster anti-system sentiments. While newspaper reporting was primarily neutral or positive, articles that dealt with government performance contained significantly more criticism and they varied considerably in presentation from paper to paper. But many newspapers apparently showed a great deal of restraint in their reporting of what was obviously very damaging news about the functioning of political leaders. Moreover, the criticism that was conveyed to the public was generally directed far more at office holders than at political norms or institutions, and it almost always originated with a political figure featured in the article.

The substantial variation we found in the amount of criticism among newspapers in the government evaluation domain challenges the notion that the large, prestigious papers set a uniform tone for all news reporting. Given that media content differs across newspapers and that these differences matter, what might account for them? Perhaps some editors set out to be particularly critical—in an attempt to fulfill their function as adversaries—while others—having decided it was time to put Watergate behind us—may have pursued a deliberate policy of avoiding critical stories. It is surprising to find so many newspapers with low levels of criticism in stories about the performance of national government during an era that witnessed such tumultuous events. Then again, perhaps newspaper differences are unplanned consequences of systematic differences in reporter recruitment or newsroom routines. Whatever the answer, we hope that further research on decision making within media organizations may be spurred by the evidence that adversely critical news content has a substantial impact on political attitudes.

The analyses reported here also demonstrate the substantive benefits derived from the methodological innovations incorporated in the 1974 CPS study. For years researchers employing national survey data have relied solely upon the respondent's reported self-perception when making inferences about the political impact of real-world variables, such as candidates, events, or the media. This study has attempted to overcome that methodological limitation by directly incorporating in the analysis a measure of newspaper content as an assumed real-world cause of attitudes toward the government. By merging content analysis and survey data, we have been able to examine the relationships between individual political attitudes and the media content to which the individual was exposed.

Methodologically, our results underscore the limitations of media research that depends solely upon exposure measures as independent variables. If media content varies, then the consequences of exposure will also vary. Thus, while we have found political trust and efficacy more consistently related to newspaper exposure than to television exposure, the absence of television content data precludes any conclusive statements about its impact relative to that of newspapers.

Finally, our analysis helps clarify the distinction between the concepts of trust and efficacy. Contrary to previous writing, distrust of government does not primarily reflect disaffection from current incumbents but denotes instead dissatisfaction with governmental policy performance and the direct impact of criticism in the media; it would thus appear to be an early indicator of accumulating diffuse discontent directed first at authorities and policies and only later at institutions or regime norms. But even as these negative sentiments are directed at institutional norms, they do not reflect a desire to replace our system of government with a different set of political arrangements, but represent at this point a decided discontent with the *malfunctioning* of the government. Accordingly, trust represents the feeling that political leaders are operating government in accordance with certain normative expectations, such as honesty, competence, equity and efficiency. The sense of efficacy, on the other hand, represents the belief that institutional arrangements are such that government will respond to the expressed desires of the people.

Consequently, it is not surprising to find the degree of media criticism more strongly related to political cynicism than to feelings of inefficacy. Apparently, the public's evaluations of government performance flow rather directly from the image of the political process as seen through the prism of newspaper reporting. Evaluations of the political system and its institutions, however, are less directly attributable to media effects; they depend on the degree of attitudinal buffering surrounding people's beliefs in prevailing regime norms. Only through its influence on a general political distrust of authorities does the media have an indirect influence on evaluations of institutional responsiveness. Nevertheless, even among politically sophisticated citizens, cynicism due to continued and frequent exposure to newspaper articles critical of political leaders eventually accumulates into dissatisfaction with the responsiveness of institutions. Ultimately, therefore, media effects can be understood as reactions to relevant, if "mediated," political realities.

Appendix A.

Political Efficacy. Listed below are the agree/disagree items that were used in the political efficacy index. These three particular items were selected for analysis because they represent the concept of efficacy as an indicator of institutional responsiveness rather than as a measure of personal competence (Converse, 1972; Balch, 1974). Had we included the three "personal" efficacy items that are usually a part of the political efficacy index, the already difficult task of sorting out the causal sequence relating feelings of efficacy and media usage would have become even more complicated. If the sense of efficacy were taken to be equal to personal competence, or the ability to understand politics, we would not have been able to determine whether a sense of competence leads to media usage or vice versa.

a. I don't think public officials care much what people like me think.

b. Generally speaking, those we elect to Congress in Washington lose touch with the people pretty quickly.

c. Parties are only interested in people's votes but not in their opinions.

Trust in Government. Political trust was operationally defined with the standard SRC-CPS trust in government scale which was constructed from the items listed below. The five items form a Guttman scale with scores that range from 1 = most trusting to 6 = least trusting or cynical. The scale score represents the number of items to which the individual gave *distrusting* responses (as underlined below). Thus a score of 6 indicates distrusting responses to all five questions, whereas a score of 1 indicates that trusting responses were given to all five scale items.

a. Do you think that people in the government waste *a lot* of the money we pay in taxes, waste some of it, or don't waste very much of it?

b. How much of the time do you think you can trust the government in Washington to do what is right—just about always, or *only some of the time?*

c. Would you say the government is pretty much run by a *few big interests* looking out for themselves or that it is run for the benefit of all the people?

d. Do you feel that almost all of the people running the government are smart people who usually know what they are doing, or do you think that quite a few of them *don't seem to know what they are doing?*

e. Do you think that *quite a few* of the people running the government are a little crooked, not very many are, or do you think hardly any of them are crooked at all?

Appendix B. Equations Predicting Cynicism and Inefficacy

	Unstandardized Regression Coefficients for:			
	Cynicism			Inefficacy
	Total	Democrats	Republicans	Total
Grade School				
Critical Content (x_1)	.030	.020	.034	.011
(Content) (Medium Exposure) (d_1x_1)	.076	.063	.073	.023
(Content) (High Exposure) (d_2x_1)	.066	.068	.062	.018
Medium Exposure TV (d_3)	1.026	1.070	1.197	–.916
High Exposure TV (d_4)	.983	1.958	1.000	–.429
Medium Exposure Papers (d_1)	–1.930	.927	–3.440	–.434
High Exposure Papers (d_2)	1.665	1.102	.302	–.695
Constant	1.24	1.56	1.31	1.67
R	.51	.46	.62	.34
High School				
Critical Content (x_1)	.013	.022	.004	.002
(Content) (Medium Exposure) (d_1x_1)	.050	.043	.040	.003
(Content) (High Exposure (d_2x_1)	.044	.047	.042	.010
Medium Exposure TV (d_3)	.463	.884	.346	.006
High Exposure TV (d_4)	.708	.428	.624	.119
Medium Exposure Papers (d_1)	–1.487	–.506	–.181	–.572
High Exposure Papers (d_2)	–1.510	–1.449	–.597	–1.153
Constant	3.24	2.61	1.94	1.77
R	.29	.27	.33	.26
College				
Critical Content (x_1)	.015	.018	.014	.004
(Content) (Medium Exposure) (d_1x_1)	.055	.044	.041	.004
(Content) (High Exposure) (d_2x_1)	.034	.046	.036	.011
Medium Exposure TV (d_3)	–.322	–.577	–.549	.124
High Exposure TV (d_4)	–.111	–.044	–.346	.103
Medium Exposure Papers (d_1)	–1.835	–2.324	–1.566	–.961
High Exposure Papers (d_2)	–.636	–2.141	–.545	–.431
Constant	3.56	3.24	3.00	1.71
R	.26	.34	.37	.26

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Factions in Committees: The U.S. House of Representatives*

GLENN R. PARKER
SUZANNE L. PARKER
Miami University

This study undertakes a major reanalysis of the decision-making processes in eight committees of the U.S. House of Representatives–Agriculture, Education and Labor, Interior and Insular Affairs, International Relations, Interstate and Foreign Commerce, Post Office and Civil Service, Public Works and Ways and Means. While past studies have relied mainly on interviews, this study uses a data base composed of recorded committee roll-call votes. The analysis consists of a Q-*component analysis to determine voting blocs or factions; an* R-*component analysis to discover issue dimensions; and step-wise regression and correlation utilizing demographic, political and electoral characteristics to define the nature of the voting blocs. The results are then compared with previous analyses of the same eight committees to determine what changes, if any, have occurred since the committees were last analyzed. The findings suggest that partisanship and ideology have become stronger influences on committee decision making than previously noted.*

In his classic study, *Congressmen in Committees,* Richard Fenno provides a conceptual schema for analyzing and comparing committee decision making. Although there are several facets to his schema, one important component is the notion of environmental constraints: "Every House committee inhabits an environment in which nonmembers seek to persuade committee members to act in ways the nonmembers deem necessary or desirable" (Fenno, 1973, p. 15). Fenno identifies four prominent influences which exist in the environment of every House committee: the executive branch, the party, clientele groups, and the House itself. These influences do not have the same impact on each committee *or* on each member.

As a result of the differing impact of these environmental influences, cleavages or divisions develop within committees. For instance, partisan differences within the committee are intensified when the influence of the party and/or the administration are salient within the committee's environment. Similarly, clientele groups often cultivate "friends" among committee members to offset the influence of other interest groups or nonsupportive committee members. This leads to the formation of clientele supporters within the committee. Divisions or differences within the House are also likely to appear within committees. Massive membership turnover in the House in the last several years has enhanced the ideological differences *among Democrats* as well as *between the parties:*

> The decrease in conservatism among Southern Democrats, coupled with the substantial increase in the *number* of Northern Democrats has had two important effects on the Democratic party in the House: the ideological character of the party has shifted somewhat to the left, and a significant gap now exists between Democrats who were members in the 91st Congress and members who entered since then. Moreover, at the same time that the Democrats have shifted somewhat to the left, the average conservative coalition score of the Republicans had increased, so that the ideological gap between the parties has also increased (Ornstein and Rhode, 1976, p. 4).

Such membership changes may sharpen the historic cleavages between liberal Democrats and conservative Democrats and Republicans.

The cleavages generated by these environmental influences create factions—pockets of committee support and/or opposition—within the committee. Salient cleavages should be present at important stages in committee decision making. The purpose of this study is to define the factional alignments in committee decision making by analyzing committee roll-call votes, and in the process, illuminate the saliency of particular environmental influences within selected House committees.

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Data and Methods

This analysis is based on a subset of roll-call votes in eight congressional committees during the 93rd and 94th Congresses. These committees span the entire range of committee types: exclusive (Ways and Means), semi-exclusive (Agriculture, Education and Labor, International Relations, Interstate and Foreign Commerce, Post Office and Civil Service, Public Works), and non-exclusive (Interior and Insular Affairs).[1] These eight House committees were selected for comparative study because their decision-making processes have been extensively studied.

There are significant differences between our analysis and past studies of these committees. This study is based on recently released committee roll-call votes. In contrast, past studies have placed almost total reliance on interview protocols in interpreting committee decision-making patterns. Since our inquiry uses a different source of data *and* a different period, our conclusions serve to extend and revise present understanding of committee decision making.

Procedural votes have been excluded from the analysis because of their natural partisan complexion;[2] the analysis, therefore, is based on votes on substantive amendments to bills considered by the committee. Roll calls where at least 90 percent of the committee members vote on the same side are removed from the analysis because our measure of vote agreement among committee members—product-moment correlation coefficient—can be severely distorted by skewed distributions. Since the uneven work load across committees results in few roll calls in some committees during the span of this analysis, we have pooled committee votes for both the 93rd and 94th Congresses. Aggregating the votes over the four-year period (1973–1976) insures that the defined factions demonstrate some stability. We have ignored intersession analyses because they tend to be substantively and statistically unreliable. The number of votes in the analysis of factions within each committee is presented in Table 1.

In addition to eliminating procedural and near-unanimous roll calls, we have restricted our pool of committee members to those who have: (1) voted on at least 70 percent of the substantive committee roll calls; and (2) served in both Congresses.[3] This requirement does not appear to be overly restrictive. In most cases, our criteria include a substantial proportion of *continuing* committee members: in five of the committees more than 80 percent of the committee members serving in both the 93rd

Table 1. Number of Roll Calls Used in the Analysis of Factions Within Congressional Committees

Committee	Substantive Roll Calls		Total Votes
	93rd Congress	94th Congress	
Agriculture	30	70	100
Education and Labor	60	32	92
Interior and Insular Affairs	40	45	85
International Relations	25	19	44
Interstate and Foreign Commerce	43	91	134
Post Office and Civil Service	13	15	28
Public Works	12	25	37
Ways and Means	68	178	246

Source: Recorded votes in eight House Committees (1973–1976).

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[1] Since most of the work of the Appropriations Committee is done in subcommittees, there are few substantive roll-call votes in the full committee. Therefore, Appropriations was excluded from the analyses. This classification schema can be found in Tacheron and Udall (1970, pp. 157–60).

[2] We have classified the following votes as procedural and have excluded them from analysis: (1) votes of reconsideration; (2) votes about rules (committee); (3) points of order; (4) suspension of rules; (5) referral of bills to subcommittees; (6) recommittal votes; (7) instructions on handling bills on the House floor; (8) discharging a subcommittee from considering a bill; (9) postponing action on a bill; (10) acceptance of subcommittee reports; (11) limitations on committee debate; (12) motions to table legislation; (13) motions to report legislation to the House. What remains, therefore, are the roll calls taken on amendments to legislation considered by a committee.

[3] This requirement limits the effects of factoring a product-moment correlation matrix where the correlations themselves are based upon varying numbers of votes.

and 94th Congresses satisfy the criteria; in the remaining three committees, 70 percent of the *continuing* members satisfy the conditions (Table 2).[4]

The factional alignments within a committee are analytically defined as the dimensions (factors) derived from a Q-principal component analysis of the substantive roll calls within each committee.[5] A varimax rotation is performed on each committee's factional structure to simplify the interpretation of the alignments.[6] The resulting factor loadings serve as measurements of the attachment of each committee member to the various factions within that committee. The vectors of factor loadings (one vector for each extracted factor) are the dependent variables in the analysis of each committee's voting patterns.

We also subject all substantive amendments voted upon by at least 70 percent of the committee members to an R-component analysis. To simplify and sharpen the interpretation of these dimensions, we have selected only those votes which are strongly correlated with an underlying issue dimension (any vote with a factor loading $> .82$).[7] This restricted pool of issues is subsequently refactored into the same number of extracted issue dimensions, and factor scores which summarize a committee member's position on the various issue dimensions are obtained. These factor scores are referred to as *issue-dimension scores.*[8]

Table 2. Evaluating the Criteria for the Selection of Committee Members in the Analysis

Committee	Number of Continuing Members	Number of Members in the Analysis
Agriculture	20	18 (90%)
Education and Labor	25	21 (84%)
Interior and Insular Affairs	26	19 (73%)
International Relations	29	20 (69%)
Interstate and Foreign Commerce	27	23 (85%)
Post Office and Civil Service	15	11 (73%)
Public Works	26	25 (96%)
Ways and Means	18	16 (89%)

Source: Committee members who have voted on at least 70 percent of substantive committee roll calls and who have served in both the 93rd and 94th Congresses.

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[4] The *total* number of committee members included in this analysis (*new and continuing* members) represents better than 70 percent of the committee membership in seven House committees; the subset of members we analyzed in the International Relations Committee comprise 65 percent of the committee's total membership.

[5] For an excellent non-technical treatment of the application of the Q-technique to legislative roll-call analysis, see Grumm (1963, esp. pp. 338–40). Our analysis follows the strategy developed by Grumm for identifying legislative voting blocs from a Q-factor analysis. Generally, we have retained all components with an eigenvalue >1.00.

[6] Clearly, a varimax rotation ignores the empirical correlations among factors, and therefore, many obscure important relationships within a committee's factional structure. We have evaluated the effects of ignoring the relationships among factors (factions) by examining the correlations among a committee's factions which appear if we allow the factors to assume a fairly oblique (correlated) solution. The analysis of the interfactor correlations resulting from oblique rotation reveals few correlated factors. The strongest factional relationships are generally moderate in strength. Further, the correlated solutions do not significantly *alter,* or *improve upon,* the simplicity of the factional structure determined by a varimax rotation. Therefore, we have retained the orthogonal rotation—varimax—of the principal component analysis of committee votes for the following reasons: (1) oblique rotations fail to improve upon the "simple structure" identified by the orthogonal rotations; (2) orthogonal rotations are conceptually and mathematically less complex; and (3) very few factors are strongly intercorrelated. Nevertheless, we utilize, when relevant, the interfactor correlations in discussing the substantive nature of the relationships among factions within a committee.

[7] In extracting the issue dimensions, we eliminate those factors which fail to account for at least 10 percent of the variation in committee voting. This cut-off point delineates the reliable from the unique factors.

[8] The factor scores are computed as a weighted product of the existing data:

Factor Score =

$$\left(\frac{\text{number of committee votes}}{\text{number of non-missing committee votes}}\right) * \Sigma_i F_i Z_i$$

Issue-dimension scores are used as explanatory variables in assessing the independent contributions of political issues to the organization of factions: they measure the extent to which issue dimensions account for unexplained variation in factional alignments. To evaluate the impact of issues on the factional structures, we formulate regression equations representing the three best predictors of each committee faction. The issue-dimension scores are then entered into these equations to explain the residual variance; the impact of issues is evaluated in terms of the increase in explained variation (R^2) resulting from the inclusion of issues in the equations.[9]

In the tables describing the factional alignments, we have included factor loadings less than .50 *only* where we feel they will clarify the interpretation of the factions. Displaying the factor loadings in this manner will illuminate the salient features of the factional structure within a committee. We have also designated with asterisks committee members who were present on the committee when it was previously studied. This information provides some indication of the impact of membership turnover on changes in committee decision making.

We originally used three types of independent variables to identify the nature of factional alignments: constituency demographics (e.g., percent black, median family income), electoral forces (e.g., 1972 and 1974 congressional district vote), and political influences (e.g., group ratings, conservative coalition scores, presidential support scores). Since neither constituency demographics nor electoral forces proved useful in identifying committee factions, these variables have been eliminated from the tables of correlations in this article.

The *Q*-component analysis delineates the voting blocs within the committee and provides an opportunity for a visual interpretation of the factional structure of the committee. The accuracy and validity of a visual interpretation of a committee's factional structure can be corroborated by examining the correlates of individual committee factions. The product-moment correlations between group ratings,[10] partisanship scores and the individual factions provide a basis for inferences regarding the underlying nature of a committee's factional structure. This procedure also provides additional specification to the visual interpretations.

In order to enhance the reliability of the interpretations, we rely upon multiple measures to describe the factions. Our interpretations are based upon the consistency in the patterns of correlations between factional alignments and diverse group ratings. Conceptually similar and strongly intercorrelated variables are treated as measurements of the same underlying dimension. We feel that this strategy is preferable to interpreting individual ratings, which tend to be less reliable than multiple-item measures. We are also reluctant to reduce the variables to a few scales because some of the specificity of a group's interests or concerns is obscured. Our analysis attempts to strike a balance between these two approaches in order to maximize the benefits of each: our interpretation of factions relies upon the patterns which appear in the correlations. At the same time, we display the correlation for each group to allow the reader to examine the different impact of individual group ratings.

One final methodological caveat is in order: we urge caution in interpreting the factional structure of committees where few roll calls have occurred because these factional align-

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Where F_i is the factor score coefficient, Z_i is the standardized variable, and the summation occurs over all non-missing votes. We have replaced absences with the mean vote for the issue where a committee member has missed less than one-third of the votes; otherwise, the committee member's issue-dimension score is defined as missing data. These calculations are necessary due to the uneven vote participation of committee members.

[9] Issue dimensions are the last variables to enter the equation because they represent the unique contribution which issues make to the organization of committee factions. Therefore, we need to remove that amount of variation which can be explained by the common behavioral influences of party, ideology, and constituency; the residual variation represents the amount of variation in the committee's factional alignments which these common legislative influences cannot explain. Clearly, party, ideology, and constituency may be associated with the substantive aspects of an issue. Still, these variables are conceptually simpler and more reliable than issue dimensions which change from committee to committee and, perhaps, year to year. Descriptions of the regression equations are illustrated in the convention version of this article.

[10] In an effort to insure the stability of the committee factions, we have examined the factional alignments among new committee members–those assigned to the committee during the 94th Congress. All of the reported factional structures appear to be stable. New committee members join the same factional alignments that exist among continuing committee members. For a discussion and description of the factional alignments among new committee members, see the extended (convention) version of this article.

ments may be unreliable. Finally, we are painfully aware of the frailties of the data, and the limitations they place on our conclusions. Throughout the analysis, we have taken precautions to insure the robustness of our findings; conclusions and interpretations which are only weakly supported by the data have been ignored.[11]

Analysis

Agriculture Committee. Four factions appear in the Agriculture Committee (Table 3). Committee Democrats divide into northern (Faction 1) and southern (Faction 2) Democrats; Republicans form the other major committee faction (Faction 3). The remaining committee members–Melcher (D-Montana) and Jones (D-Tennessee)–form a factional doublet (Faction 4), which tends to vote with the northern Democrats more frequently than with the southern Democrats; Faction 4 has an interfactor correlation of .44 with Faction 1 and .32 with Faction 2. The four committee factions appear to reflect differing levels of liberalism and party support. For instance, we can order the factions in terms of an ideological continuum. Republi-

Table 3. Factions Within the House Agriculture Committee

	Factions				
Committee Members	F_1	F_2	F_3	F_4	h^2
Thomas S. Foley (D-Washington)	.85				.81
Joseph P. Vigorito (D-Pennsylvania)	.80				.72
Bob Bergland (D-Minnesota)	.74				.76
Jerry Litton (D-Missouri)	.86				.85
George E. Brown (D-California)	.54	–.50			.68
**W. R. Poage (D-Texas)		.81			.72
Walter B. Jones (D-North Carolina)		.80			.76
Dawson Mathis (D-Georgia)		.67			.66
David Bowen (D-Mississippi)		.52		(.44)	.66
William C. Wampler (R-Virginia)			.73		.73
Keith G. Sebelius (R-Kansas)		(.45)	.62		.68
Charles Thone (R-Nebraska)			.80		.74
James P. Johnson (R-Colorado)	(–.45)		.61		.59
Edward R. Madigan (R-Illinois)			.75		.72
Steven D. Symms (R-Idaho)	–.54		.55		.80
Ed Jones (D-Tennessee)				.83	.77
John Melcher (D-Montana)	(.47)			.74	.85
Paul Findley (R-Illinois)			(.46)	–.59	.62

Correlates of Factional Alignments

	F_1	F_2	F_3	F_4
ADA Scores	.73*	–.80*	–.71*	.37
COPE Scores	.89*	–.45	–.88*	.64*
NFU Scores	.89*	–.65*	–.84*	.59*
ACA Scores	–.84*	.76*	.73*	–.38
Party Unity (Voting with Democrats) Scores	.88*	–.60*	–.86*	.52*
Conservative Coalition Scores	–.79*	.77*	.75*	–.38
Presidential Support Scores	–.82*	.42	.81*	–.71*
CCUS Scores	–.82*	.69*	.77*	–.49
Issue-Dimension I Scores	.84*	–.77*	–.83*	.38
Issue-Dimension II Scores	.49	.25	–.45	.66*
Unique Contribution of Issue Dimensions	.13*	.01	.20*	.05

Source: U.S. House Committee on Agriculture roll-call votes (1973–1976).

*Statistically significant at the .01 level.

**Committee member during 85th Congress.

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[11] The group ratings are:

Americans for Democratic Action (ADA)
Committee on Political Education (COPE)–AFL-CIO
National Farmers' Union (NFU)
Americans for Constitutional Action (ACA)
U.S. Chamber of Commerce (CCUS)

These ratings were obtained from *Congressional Quarterly's Weekly Report* (94th Congress).

cans (Faction 3) and southern Democrats (Faction 2) anchor the conservative end, while northern Democrats (Faction 1) form the liberal side of the continuum. Melcher and Jones (Faction 4) fall between these ideological extremes representing a more moderate alignment of committee Democrats. In sum, a visual examination of the factional alignments suggests that partisan and ideological influences structure the voting patterns of committee members.

The correlates of the factional alignments are consistent with our visual interpretation of the committee's voting patterns: ideological (ADA, ACA, COPE, conservative coalition, CCUS, and NFU scores) and partisan measures (party unity and presidential support scores) consistently bear the strongest relationships to the factions (Table 3). Unfortunately, ideology and partisanship are virtually impossible to disentangle; for example, party unity scores are correlated above .9 with the ideological measures which are also highly intercorrelated. We are unable, therefore, to determine which of the two variables, ideology or party, is the most influential in committee decision making.

Although issues appear to contribute to the explanation of the alignments of members of Factions 1 and 3, their substantive contribution is modest: 80 percent of the variation in these two factions can be accounted for by other independent variables. Issues related to the Food Stamp Program and the Sugar Act have the strongest factor loadings with the first issue dimension (I), while amendments to the General Farm Bill have a similar relationship to the second issue dimension (II). Since issue dimension I scores are closely associated with party unity ($r = .90$) and measures of liberalism (ADA scores, $r = .94$; conservative coalition scores, $r = -.96$), the first issue dimension appears to reflect the partisan and ideological differences within the committee. Issue dimension II scores fail to reveal a similar pattern of correlations.

The factional structure within the Agriculture Committee reflects the environmental influences of the parties and the House on the committee. The effects of party are evident in the partisan composition of the factional alignments. Ideology creates further fragmentation among the Democrats. The liberal-conservative split among House Democrats appears in the division between committee Democrats (Factions 1 and 2).

Charles Jones' study of the House Agriculture Committee reaches somewhat different conclusions about the decision-making forces within this committee (Jones, 1961, p. 367). He suggests that constituency commodity interests are a primary influence on committee decision making.[12] Further, our analysis reveals a greater degree of ideological voting than Jones observes.

There are several factors which could account for the differences between our findings and those of Jones. For instance, a parallel between commodity interests and partisanship could obscure the impact of commodity interests on decision making. It may also be that commodity interests are more important at earlier stages in the decision-making process. Our data were collected near the final phases of decision-making where partisanship becomes a more important factor. As Jones says, "Representatives, whether or not affected by the legislation, tend to support their party's position more as the action moves beyond the basic working level, and most at the final vote" (1961, p. 367). Finally, the dramatic change in committee personnel since Jones' study also may account for some of the differences in the findings: W. R. Poage is the *only* member in our analysis who was also on the committee during the 85th Congress (Table 3—asterisks).

Education and Labor Committee. The Education and Labor Committee is divided into three factions (Table 4): labor Democrats (Faction 1), liberal Democrats (Faction 3), and Republicans (Faction 2). The importance of party affiliation is evident in the linkages between liberal and labor Democrats—the interfactor correlation (oblique rotation) is .56. In addition to their partisan similarities, Factions 1 and 3 appear to share similar ideological perspectives—support of labor and liberalism. These similarities are evident in the overlap of Democrats Hawkins, Ford, Mink, Brademas, O'Hara, Biaggi, and Lehman in two or more of the factions.

Our visual interpretation of the committee's factional structure is consistent with the correlates of the alignments. The ideological and partisan measures reveal the strongest correlations with the individual factions (Table 4—correlates). The pattern of the correlates suggest that Faction 1 is ideological in nature. Furthermore, there is reason to suspect that this faction also reflects the influence of labor on committee decision making: COPE scores have the highest correlation with this factor—.88. This labor orientation is also evident in the fact that the chairpersons of the labor subcommittees (1973–1976) have the strongest attachments to this faction (Thompson, Dent, and Daniels). This information leads us to suspect that labor may be a dominant interest in this faction.

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[12] A closer examination of the bloc of southern Democrats reveals that each chairs a subcommittee which deals with a different commodity. These are the same commodities which Jones observes are favored when Democrats control the committee (1961, p. 360). While we suspect that this faction reflects clientele influence (commodity interests) we are reluctant to describe it as a clientele faction in the absence of direct measurement of the attachment of committee members to the individual commodities.

Partisanship and ideology have the strongest correlations with Factions 2 and 3, respectively. The intercorrelations among our independent variables demonstrate the overlap of ideology and partisanship among committee factions. This overlap is evident in the high correlations between ADA scores and party unity scores (.93).

Issues are relatively unimportant in defining the factional alignments. The issue dimension I scores do not contribute significantly to the explanation of the three committee factions (Table 4). This is probably due to the strong association of this issue dimension (education related issues) with attributes of the factions—i.e., partisanship (r = .96), ideology (ACA scores, r = –.91) and labor support (r =.97).

The Education and Labor Committee's environment is sensitive to partisan and ideological forces. The overlap of party and ideology is

Table 4. Factions Within the House Education and Labor Committee

	Factions			
Committee Members	F_1	F_2	F_3	h^2
**Carl D. Perkins (D-Kentucky)	.73			.60
**Frank Thompson (D-New Jersey)	.82			.85
**John H. Dent (D-Pennsylvania)	.87			.84
**Dominick V. Daniels (D-New Jersey)	.89			.90
**John Brademas (D-Indiana)	.77		(.40)	.79
**James G. O'Hara (D-Michigan)	.60		(.49)	.67
Joseph M. Gaydos (D-Pennsylvania)	.88			.87
Mario Biaggi (D-New York)	.71		(.45)	.70
William Lehman (D-Florida)	.64	.51		.69
**Lloyd Meeds (D-Washington)		.52		.53
**Albert H. Quie (R-Minnesota)		–.84		.85
**Alphonzo Bell (R-California)		–.90		.85
**John N. Erlenborn (R-Illinois)		–.82		.91
Marvin L. Esch (R-Michigan)		–.81		.70
Edwin D. Eshleman (R-Pennsylvania)		–.75	(–.46)	.79
**Augustus F. Hawkins (D-California)	(.49)	.57	.55	.86
**William D. Ford (D-Michigan)	(.45)	(.43)	.52	.66
**Patsy T. Mink (D-Hawaii)	(.43)	(.41)	.74	.90
**Phillip Burton (D-California)			.64	.70
William (Bill) Clay (D-Missouri)			.87	.94
Shirley Chisholm (D-New York)			.77	.80

Correlates of Factional Alignments

	F_1	F_2	F_3
ADA Scores	.64*	.88*	.78*
COPE Scores	.88*	.93*	.79*
NFU Scores	.81*	.88*	.79*
ACA Scores	–.76*	–.91*	–.82*
Party Unity (Voting with Democrats) Scores	.80*	.95*	.81*
Conservative Coalition Scores	–.63*	–.86*	–.88*
Presidential Support Scores	–.74*	–.81*	–.80*
CCUS Scores	–.77*	–.87*	–.86*
Issue-Dimension I Scores	.88*	.96*	.82*
Unique Contribution of Issue Dimension	.00	.03	.00

Source: U.S. House Committee on Education and Labor roll-call votes (197

*Statistically significant at the .01 level.

**Committee member during 89th Congress.

evident in the substantial bipolarity of the Republican faction (Faction 2)—conservative Republicans opposing liberal Democrats. When there is a substantial congruence between party and ideology, strong bipolar factors are apt to appear. We can attribute the ideological warfare to the influence of the House within the committee's environment—the ideological polarization between the parties in the House. There is also evidence to suggest that a clientele group (labor) has an impact on decision making.

The existence of ideological and partisan factions within this committee is consistent with Fenno's description of committee decision making: "Partisan-ideological warfare is the normal condition of Education and Labor" (1973, p. 76). Fenno also finds that organized labor is influential in committee decision making. The influence of labor (Faction 1) is also suggested by our findings. Our analysis appears to support Fenno's conclusions that ideology, party, and organized labor are important influences on committee decision making.

Interior and Insular Affairs. The members of this committee divide into two *major* factions (Factions 1 and 2); the remaining two factions (3 and 4) are of minor significance since few members consistently vote with them. The *major* factions appear to reflect ideological and partisan differences among committee members. The partisan-ideological conflict is manifested in the strong bipolarity of Faction 1 where Republican conservatives like Steiger, Young, and Clausen consistently oppose the

Table 5. Factions Within the House Interior and Insular Affairs Committee

Committee Members	Factions F_1	F_2	F_3	F_4	h^2
Don H. Clausen (R-California)	−.73		(.43)		.72
**Harold T. Johnson (D-California)	−.70				.55
Keith G. Sebelius (R-Kansas)	−.67		(.42)		.77
Sam Steiger (R-Arizona)	−.88	(−.38)			.94
Donald E. Young (R-Alaska)	−.84				.75
John F. Seiberling (D-Ohio)	.83				.78
**Jonathan B. Bingham (D-New York)	.85	(.46)			.93
**Phillip Burton (D-California)	.84	(.47)			.94
Robert W. Kastenmeier (D-Wisconsin)	.77	.51			.87
Manuel Lujan (R-New Mexico)	−.66	−.53			.79
Patsy T. Mink (D-Hawaii)	.80	(.48)			.87
**Morris K. Udall (D-Arizona)	.73	.53			.84
Joseph P. Vigorito (D-Pennsylvania)	(.38)	.79			.82
**Lloyd Meeds (D-Washington)	(.39)	.82			.84
John Melcher (D-Montana)		.77			.61
**Teno Roncalio (D-Wyoming)		.64		.57	.76
**Joe Skubitz (R-Kansas)	(−.48)	−.55	(.45)		.74
**Roy A. Taylor (D-North Carolina)			.80		.65
Philip E. Ruppe (R-Michigan)				.82	.79

Correlates of Factional Alignments

	F_1	F_2	F_3	F_4
ADA Scores	.84*	.84*	−.55*	−.35
COPE Scores	.78*	.88*	−.45	−.32
NFU Scores	.87*	.87*	−.53*	−.21
ACA Scores	−.84*	−.88*	.51*	.35
Party Unity (Voting with Democrats) Scores	.81*	.84*	−.43	−.34
Conservative Coalition Scores	−.86*	−.79*	.65*	.24
Presidential Support Scores	−.76*	−.83*	.62*	.33
CCUS Scores	−.87*	−.80*	.49	.30
Issue-Dimension I Scores	.96*	.92*	−.45	−.23
Unique Contribution of Issue Dimension	.06	.00	.02	.00

Source: U.S. House Committee on Interior and Insular Affairs roll-call votes (1973–1976).

*Statistically significant at .01 level.

**Committee member during 89th Congress.

alignment of Democratic liberals Burton, Mink, and Bingham. Faction 2 captures committee partisanship. We observe, for example, that Democrats like Burton, Bingham, Kastenmeier, and Udall align with Faction 2, and are opposed by Republicans like Steiger, Lujan, and Skubitz. As in the Education and Labor Committee, there is considerable overlap in partisanship and ideology within the committee. It is not surprising, therefore, that the *major* factions (1 and 2) are related ($r = .46$).

The ideological and partisan nature of the factional structure is also evident in the correlates of the alignments (Table 5). Indicators of partisanship and ideology are consistently the strongest correlates of committee factions. Taken individually, each of these variables explains at least 60 percent of the variation in the attachments of committee members to Factions 1 and 2. The correlates of the two minor Factions 3 and 4 fail to achieve a similar level of importance, but the ideological and partisan patterns in the correlations continue even here.

Although it might be tempting to construe Faction 1 as reflecting the consumer versus business (or "user versus conservationist") battles that rage elsewhere, such an inference is problematic. Since CCUS scores are strongly correlated with other partisan (party unity scores, $r = -.91$) and ideological (ACA scores, $r = .95$) measures, it is difficult to define Faction 1 solely in terms of its posture toward business interests. It seems more likely that pro- and anti-business orientations are a function of underlying ideological and partisan commitments. Once again, we find that ideological and partisan influences on the committee's factional alignments are difficult to separate. The correlation between party unity and ADA scores is .97 and other ideological and partisan indicators are similarly intercorrelated. These relationships capture the ideological and partisan polarization within the committee.

Issue dimension 1 is of minor substantive importance in the organization of the committee factions. Despite the statistical improvement in explaining the variation in attachments to Faction 1, the substantive contribution of the issue dimension is a mere 6 percent (Table 5). This issue dimension reflects votes on land-use policy and surface mining. Such issues seem to incite partisan and ideological sentiment as illustrated by the close association of issue dimension I scores with ideology and party; these scores have a correlation of $-.90$ with ACA scores, and .87 with party unity scores. Thus, issues may fail to exercise an independent impact on the factional alignments because of their close association with the existing ideological and partisan divisions within the committee.

The environment of the Interior Committee appears to be dominated by the influences of the House and the political parties. The congruence between party and ideology, evident in the bipolarity of Faction 1, reflects the ideological polarization of the parties in the House. The factional structure also exhibits clear partisan cleavages (Faction 2) with Republicans consistently opposing alignments of Democratic committee members. Thus, political parties and their ideological polarization within the House are important influences on the committee's decision-making environment.

Fenno's conclusions concerning Interior and Insular Affairs differ from ours. He describes committee divisions more in terms of "East versus West, than Democrats versus Republicans" (Fenno, 1973, p. 59) and this East-West division within the committee mirrors the conflict between committee conservationists and commercial users. Fenno also places less emphasis on partisanship in committee decision making: "For a group whose strategic premises involved the close adherence to clientele desires, partisanship is not relevant" (1973, p. 91).

Although unlike us Fenno finds a conservationist-commercial user division in the Interior Committee, it may be that this division can be subsumed under a broader ideological framework. Liberals generally favor conservationist positions, while conservatives support commercial users' interests. In short, ideology may be a more abstract, organizing characteristic of committee factions.

The greater emphasis on partisanship in our study may be owing to the nature of the issues that are *now* considered by the Interior Committee. One committee member interviewed by Fenno attributed the lack of partisanship to the type of issues dealt with by the committee:

> You're more likely to have geographical partisanship or regional partisanship than party partisanship. I have more in common with a Republican from Montana than with a Democrat from Florida. . . . You might have one issue a year that's partisan, a real-hot partisan issue—Hells Canyon—but most of them are so noncontroversial. It's not like . . . Education and Labor where you have these great national issues with the parties lined up on either side (1973, p. 59).

Clearly the issues *presently* handled by the committee are different. They are issues that have achieved national saliency and which divide the parties (e.g., surface mining regulations, land-use policy). In addition, there have been some changes in committee membership—only one *Republican* (Skubitz) in our analysis was a member of the committee during Fenno's analysis (Table 5—asterisks). This change in Republican membership may result in the greater partisanship we observe within the committee's factional structure. In sum, changes in the nature of issues and in committee membership (among Republicans) may account for the differences between our findings and Fenno's conclusions.

International Relations Committee. The International Relations Committee has the most fragmented factional structure of any of the committees we examine (Table 6). It is clear from Table 6 that party lines are more permeable in this committee: conservative Republicans and Democrats tend to vote together. For instance, conservative Republicans like Broom-

Table 6. Factions Within the House International Relations Committee

Committee Members	Factions					
	F_1	F_2	F_3	F_4	F_5	h^2
**Thomas E. Morgan (D-Pennsylvania)	.68					.74
**Clement J. Zablocki (D-Wisconsin)	.87					.85
**L. H. Fountain (D-North Carolina)	.73					.66
**Benjamin S. Rosenthal (D-New York)	−.70	.54		(.41)		.95
**Dante B. Fascell (D-Florida)		.95				.93
**Donald M. Fraser (D-Minnesota)		.81				.84
**Lee H. Hamilton (D-Indiana)		.74				.69
**William S. Broomfield (R-Michigan)	.61		.71			.95
Larry Winn, Jr. (R-Kansas)	.60		.72			.90
John Buchanan (R-Alabama)			.79			.80
Benjamin A. Gilman (R-New York)			.86			.83
Tennyson Guyer (R-Ohio)	.53		.66			.90
**Edward J. Derwinski (R-Illinois)			.74	(−.41)		.93
Jonathan B. Bingham (D-New York)	−.71					.77
Michael Harrington (D-Massachusetts)				.79		.88
Charles W. Whalen, Jr. (R-Ohio)	(−.40)			.48		.48
**Robert N. C. Nix (D-Pennsylvania)				.66	.52	.73
Lester L. Wolff (D-New York)					.60	.73
Pierre S. duPont (R-Delaware)					.87	.77
Edward G. Biester, Jr. (R-Pennsylvania)					.66	.75

Correlates of Factional Alignments

	F_1	F_2	F_3	F_4	F_5
ADA Scores	−.75*	.59*	−.74*	.67*	.19
COPE Scores	−.45	.36	.66*	.58*	.16
NFU Scores	−.58	.51*	−.58*	.63*	.19
ACA Scores	.66*	−.55*	.76*	−.63*	−.39
Party Unity (Voting with Democrats) Scores	−.57*	.53*	−.74*	.56*	.16
Conservative Coalition Scores	.74*	−.51*	.76*	−.67*	−.26
Presidential Support Scores	.56*	−.50*	.82*	−.61*	−.20
CCUS Scores	.64*	−.37	.71*	−.54*	−.30
Issue-Dimension I Scores	.63*	−.03	.50*	−.56*	.29
Issue-Dimension II Scores	−.59*	.85*	−.74*	.64*	.43
Issue-Dimension III Scores	−.29	.34	.37	.02	−.43
Unique Contribution of Issue Dimensions	.03	.24*	.22[a]	.11	.26*[b]

Source: U.S. House Committee on International Relations roll-call votes (1973–1975).

*Statistically significant at .01 level.

**Committee member during 89th Congress.

[a] The contribution of issue-dimension I scores is inconsequential and is ignored in the calculation of R^2 for issue dimensions or the test of significance.

[b] The contribution of issue-dimension II scores is inconsequential and is ignored in the calculation of R^2 for issue dimensions or the test of significance.

field, Winn, and Guyer join the conservative Democrats like Fountain and Zablocki (Faction 1) in voting against liberals like Whalen, Bingham, and Rosenthal. Two other factional alignments also appear to be organized along ideological lines. Faction 4 appears to be a small alignment of liberals which is defined by Harrington (D-Massachusetts). The more moderate committee faction (5) is led by Republican duPont. The positive attachments of Republicans to both Factions 1 and 3 suggest that both factions have similar policy interests–i.e., support for administration positions in foreign affairs.

The organizing characteristic of Faction 2 is more difficult to determine. Fenno (1973, p. 284) reports the emergence of a group of activist subcommittee leaders as a result of reforms introduced into the 92nd Congress. One of the consequences of these reforms is that Democrats are prevented from chairing more than one subcommittee. This has resulted in a more active subcommittee system. The small cluster of members in the faction (2) appears to fit Fenno's description: Three members of this faction (Fascell, Fraser, and Hamilton) hold subcommittee chairs. The alignment of the four members of Faction 2 may reflect opposition to the control that the executive branch exercises over the committee's environment. The vehicle for this opposition is the rejuvenated subcommittees which could develop sufficient policy expertise to formulate alternatives to administration policies. The significance of issues to the organization of this faction suggests that this bloc of subcommittee leaders may reflect tacit agreement to seek, and support, alternatives to administration policies.

Although we have dismissed partisanship as relatively unimportant to the committee's factional structure, we observe a pattern of significant correlations between the factions and measures of partisanship. In most of the factions (1, 2, and 4) this association can be attributed to the effects of ideology which are strongly related to partisanship: party unity and ACA scores are correlated at –.94. Further, our measures of ideology generally have a stronger correlation with these individual factions than do the partisan measures. Finally, the agreement between Democrats and Republicans attached to the same faction, weakens any partisan organization of the factional structure (Table 6–factions).

Issues help to explain the nature of some of the factional alignments within the committee: issue dimension scores are statistically and substantively important in three of the committee factions (2, 3, and 5, Table 6). Issue dimensions I and II are identified by foreign aid decisions, but the two dimensions differ in their focus. Issue dimension I deals with the initial question of *supplying aid* to a foreign country, while the second dimension deals with the *level of aid* supplied. The third issue dimension focuses on congressional influence in foreign affairs (e.g., War Powers Resolution, disclosure of executive agreements). The second issue dimension divides the committee factions in predictable directions: pro-administration and conservative members are more apt than their counterparts to support administration-supported aid levels.

The nature of the committee's factions suggest that the administration is a major environmental influence. The effects of the executive branch are reflected in the alignment of Republicans (Faction 3), and the vote agreement between conservative Democrats (Faction 1) and this Republican faction. In addition, the alignment of activist subcommittee leaders (Faction 2) suggests an organized challenge to the primacy of the executive branch in matters of foreign aid and policy. The ideological polarization between and within the parties in the House also finds expression within the committee's factional structure: conservative and liberal Democrats tend to oppose one another. It appears, therefore, that the administration and the House exercise considerable influence within the committee's environment. In short, our analysis reveals that the committee's factional structure is a response to issues, ideology, and the policy initiatives of the executive branch.

Our findings concerning the factional structure of this committee generally follow Fenno's description of committee decision making. The absence of cohesive party alignments supports Fenno's contention about the low level of committee partisanship: "Surely the Committee members' foreign aid policy goals and their environmental pressures are both conducive to a low level of partisanship" (1973, p. 90). In addition, our analysis locates a pro-administration contingent among committee members (Factions 1 and 3) (Fenno, 1973, pp. 71–72). Faction 2 reflects the voting behavior of an activist group of subcommittee members which Fenno describes as supportive of greater committee involvement in foreign policy decisions (1973, p. 72). The one difference between our findings and Fenno's is that our analysis reveals a greater degree of ideology than Fenno suggests.

Interstate and Foreign Commerce Committee. There is little ambiguity about the organization of the factions in the Interstate and Foreign Commerce Committee. Factions 1 and 2 are bipolar with liberal Democrats (Faction 1) opposing conservative Republicans (Table 7).[13] There appears to be an ideological and partisan organization to the factional alignments.

The correlates of the committee's alignments are consistent with this interpretation: partisan and ideological measures are strongly related to each of the factions (Table 7—correlates). As might be expected, partisanship and ideology are inextricably linked among committee members; for example, ACA and party unity scores are correlated at −.96. In fact, both measures of partisanship—party unity and presidential support scores—are correlated above .9 with the several ideological measures.

The major issue dimension within the committee is relatively unimportant in defining factions; issue dimension I reflects differences in the areas of energy policy and consumer protection (e.g., Clean Air Act amendments,

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[13] The only Democrat which violates this pattern is conservative David Satterfield (D-Virginia).

Table 7. Factions Within the House Interstate and Foreign Commerce Committee

	Factions		
Committee Members**	F_1	F_2	h^2
James T. Broyhill (R-North Carolina)	.75	(−.48)	.79
Tim Lee Carter (R-Kentucky)	.80		.75
Clarence J. Brown (R-Ohio)	.81	(−.45)	.87
Joe Skubitz (R-Kansas)	.77	(−.43)	.78
Louis Frey, Jr. (R-Florida)	.77	(−.44)	.78
John Y. McCollister (R-Nebraska)	.80	(−.40)	.88
Norman F. Lent (R-New York)	.77		.75
H. John Heinz III (R-Pennsylvania)	.64		.43
James M. Collins (R-Texas)	.63	−.66	.84
Samuel L. Devine (R-Ohio)	.70	−.59	.84
David E. Satterfield III (D-Virginia)	.67	−.52	.72
John E. Moss (D-California)	−.65	.56	.73
Brock Adams (D-Washington)	−.54	.60	.65
Bob Eckhardt (D-Texas)	−.64	.62	.79
Ralph H. Metcalfe (D-Illinois)	−.60	.69	.84
John H. Dingell (D-Michigan)	(−.40)	.56	.48
Paul G. Rogers (D-Florida)		.81	.74
Lionel VanDeerlin (D-California)	(−.48)	.58	.56
Fred B. Rooney (D-Pennsylvania)		.83	.70
Richardson Preyer (D-North Carolina)		.77	.61
James W. Symington (D-Missouri)	(−.41)	.70	.66
Charles Carney (D-Ohio)	(−.47)	.72	.74
Harley O. Staggers (D-West Virginia)	(−.44)	.78	.80

Correlates of Factional Alignments

	F_1	F_2
ADA Scores	−.94*	.87*
COPE Scores	−.90*	.91*
NFU Scores	−.92*	.94*
ACA Scores	.89*	−.93*
Party Unity (Voting with Democrats) Scores	−.94*	.96*
Conservative Coalition Scores	.90*	−.88*
Presidential Support Scores	.93*	−.92*
CCUS Scores	.88*	−.93*
Issue-Dimension I Scores	−.98*	.98*
Unique Contribution of Issue Dimension	.00	.02

Source: U.S. House Committee on Interstate and Foreign Commerce roll-call votes (1973–1976).

*Statistically significant at the .01 level.

**All committee members present during 93rd Congress.

consumer product warranties). The lack of influence of issues is probably a result of their partisan and ideological nature: Issue dimension I scores are correlated above .9 with every measure of ideology and partisanship. Therefore, the contribution which issues might make in the interpretation of the committee's factional structure is already captured by our ideological and partisan measures.

The strong bipolar nature of the factional structure reflects the congruence between party and ideology among House members—conservative Republicans oppose liberal Democrats. This suggests that the nature of the House is an important influence on committee decision making. The partisan complexion of the two factions is also clear, indicating that the political parties are also important environmental influences.

Our analysis reaches somewhat different conclusions from those of a recent study of the House Interstate and Foreign Commerce Committee (Price et al., 1975, pp. 50–55). We agree with David Price and his colleagues as to the ideological divergence within the committee, but our findings suggest greater partisanship in committee voting. These partisan alignments may be a function of the data: our roll-call data are collected near the final stages of committee deliberations. It may be, as in the Agriculture Committee, that partisanship increases as legislative action moves beyond the subcommittee level to the final vote.

Post Office and Civil Service Committee. Partisanship appears to play a major role in the organization of factional alignments within the Post Office and Civil Service Committee. Democrats define Faction 1, while Republicans have similar attachments to Faction 2 (Table 8). In addition to partisanship, ideology also structures voting blocs within the committee. For example, conservative Republicans Rousselot and Derwinski tend to oppose Faction 1, and two of the more moderate Democrats (Daniels and Hanley) tend to vote with Faction 3, creating a more moderate voting bloc.

A major exception to this pattern occurs with regard to the voting behavior of Democrats Richard White (Texas) and David Henderson (North Carolina). Both of these Democrats are ideologically conservative, but White votes with the Democrats (Faction 1) and Henderson with the conservative Republicans (Faction 2). It seems that partisan considerations are more important than ideological proximity in White's case, but less relevant to Henderson's voting.

The correlates of the factional alignments are consistent with our interpretations. Ideology and partisanship are strongly correlated with the individual factions (Table 8—correlates). These ideological and partisan influences on the factional structure are impossible to disentangle since party unity scores are correlated above .9 with each of our ideological measures.

Issues provide a statistically significant contribution to the explanation of Factions 1 and 2. The issue dimensions deal with Civil Service legislation (I); legislative, executive and judicial salaries (II); and postal rates (III). Issue-dimension III (decisions on postal rates) does more to explain the attachment of members to Faction 1 than the other issue dimensions. Issue dimensions I and II are most relevant to the organization of Faction 2; they are also most likely to have administration positions attached to them. This may account for the significance of presidential support scores in explaining Faction 2.

In sum, partisanship, ideology, and the position of the administration on issues before the committee are the most important influences on the factional structure of the Post Office and Civil Service Committee. We can interpret these relationships as reflecting the saliency of party, the executive branch, and the House within the committee's environment. These environmental influences are not as readily apparent as in other committees because of the relatively few committee members actively involved in committee decision making, and the small number of roll-call votes taken within the committee. Hence, our interpretation of the environmental influences in this committee is more tentative than in other committees where these limitations are not present.

Initially, Fenno characterized the Post Office and Civil Service Committee as a nonpartisan, nonideological committee dominated by postal clientele groups, but this description is modified in his "Epilogue" (1973, pp. 64–69, 281–83). According to Fenno, the 1966 pre-Christmas mail breakdown in Chicago "shattered the monolithic, clientele-dominated environment of the Committee" (1973, p. 282). Eventually this, and subsequent events, have served to increase the saliency of postal issues nationally. This has resulted in more administration involvement in these issues, and presidential involvement also introduces partisanship into the committee's deliberations (Fenno, 1973, pp. 282, 66).

Our conclusion about the partisan nature of committee voting is consistent with Fenno's analysis of changes within the committee—

greater presidential involvement producing more partisanship. However, our findings differ in that the linkage between partisanship and ideology in our analysis suggests an ideological component to the partisan alignments.

Public Works Committee. The alignments within this committee are clearly partisan: Republicans (Faction 1) and Democrats (Faction 2) define separate factional alignments (Table 9). We also observe a small alignment of more liberal Democrats–Studds, Abzug, and Stanton (Faction 3).

Our description of the committee's factional structure is consistent with the pattern of correlates of the factions: ideology and party are the most important explanatory variables (Table 9–correlates) in Factions 1 and 3. The distinguishing characteristic of Faction 2 is party affiliation. As in the other committees, there is a degree of partisan and ideological overlap among the committee members. ACA and party unity scores, for example, are strongly intercorrelated ($r = -.95$) and presidential support and conservative coalition scores are also collinear ($r = .91$). This collinearity prevents us from disentangling the different effects of party and ideological forces on Factions 1 and 3. However, this difficulty does not appear to cloud the interpretation of Faction 2, which contains the largest Democratic voting bloc, and is clearly a partisan faction.

Issue dimensions provide a statistically significant contribution to the explanation of the alignments of Factions 1 and 2 (Table 9). The issues underlying the dimensions deal with transportation measures (e.g., Federal Mass Transit Act) and standards to be applied in the

Table 8. Factions Within the House Post Office and Civil Service Committee

Committee Members	Factions F_1	 F_2	 F_3	h^2
**Dominick V. Daniels (D-New Jersey)	.78		(.43)	.81
Patricia Schroeder (D-Colorado)	.80			.68
William Lehman (D-Florida)	.84			.76
John H. Rousselot (R-California)	−.72	(.35)	(−.47)	.86
**James M. Hanley (D-New York)	.70		.52	.77
**Richard C. White (D-Texas)	.63	−.50		.67
**David N. Henderson (D-North Carolina)		.78		.67
**Edward J. Derwinski (R-Illinois)	(−.43)	.82		.87
**Albert W. Johnson (R-Pennsylvania)		.81		.66
**Charles H. Wilson (D-California)			.94	.92
William L. Clay (D-Missouri)			.81	.84

Correlates of Factional Alignments

	F_1	F_2	F_3
ADA Scores	.75*	−.64	.74*
COPE Scores	.85*	−.71*	.81*
NFU Scores	.81*	−.62	.83*
ACA Scores	−.81*	.66*	−.81*
Party Unity (Voting with Democrats) Scores	.89*	−.63	.74*
Conservative Coalition Scores	−.67*	.52	−.84*
Presidential Support Scores	−.80*	.71*	−.68*
CCUS Scores	−.78*	.60	−.87*
Issue-Dimension I Scores	.76*	−.90*	.76*
Issue-Dimension II Scores	.28	.41	.46
Issue-Dimension II Scores	.53	−.02	−.42
Unique Contribution of Issue Dimensions	.08*[a]	.46*[b]	.05

Source: U.S. House Committee on Post Office and Civil Service roll-call votes (1973–1976).

*Statistically significant at the .01 level.

**Committee member during 89th Congress.

[a] The contributions of issue-dimension I and II scores are inconsequential and are ignored in the calculation of R^2 for issue dimensions or the test of significance.

[b] The contribution of issue-dimension III scores is inconsequential and is ignored in the calculation of the R^2 for issue dimensions or the test of significance. Aside from the issue-dimensions, the only variable in the equation is presidential support scores.

funding of transportation projects. Nevertheless, partisan and ideological variables explain a greater proportion of the variation in the alignments. Although the committee's factional alignments may be influenced by the nature of the issues at hand, ideology and party are likely to dominate voting patterns.

It seems clear that party is the *dominant* environmental influence on committee decision making, but the small alignment of liberal Democrats within the committee's factional structure suggests that ideology may be important as well. This split among the Democrats seems consistent with the ideological cleavages within the party in the House. Hence, we suspect that the House is also a significant environmental influence on committee decision making.

This centrality which we attribute to partisanship in the organization of committee factions is consistent with James Murphy's characterization of committee decision making: "Most committee legislation will engender party disputes because committee Democrats and

Table 9. Factions Within the House Public Works Committee

	Factions			
Committee Members	F_1	F_2	F_3	h^2
Gene Taylor (R-Missouri)	.87			.88
James Abdnor (R-South Dakota)	.85			.82
Thad Cochran (R-Mississippi)	.90			.85
William F. Walsh (R-New York)	.59			.40
E. G. (Bud) Shuster (R-Pennsylvania)	.63			.47
John P. Hammerschmidt (R-Arkansas)	.87			.80
M. G. Snyder (R-Kentucky)	.90			.81
**Don H. Clausen (R-California)	.69			.50
**James C. Cleveland (R-New Hampshire)	.81			.68
**William H. Harsha (R-Ohio)	.80			.66
Gerry E. Studds (D-Massachusetts)	–.73		.59	.92
Bella S. Abzug (D-New York)	–.73		.56	.87
**Robert E. Jones (D-Alabama)		.96		.92
**James C. Wright, Jr. (D-Texas)		.71		.50
**Harold T. Johnson (D-California)		.77		.61
**David M. Henderson (D-North Carolina)		.92		.94
**Ray Roberts (D-Texas)		.79		.63
**James J. Howard (D-New Jersey)		.78		.73
Robert A. Roe (D-New Jersey)		.82		.77
Mike McCormack (D-Washington)		.92		.90
John B. Breaux (D-Louisiana)		.87		.78
Bo Ginn (D-Georgia)		.76		.58
Glenn M. Anderson (D-California)		.68	.53	.79
James V. Stanton (D-Ohio)			.78	.80
Dale Milford (D-Texas)			–.84	.81

Correlates of Factional Alignments

	F_1	F_2	F_3
ADA Scores	–.81*	.33	.73*
COPE Scores	–.82*	.61*	.71*
NFU Scores	–.77*	.51*	.72*
ACA Scores	.82*	–.47*	–.76*
Party Unity (Voting with Democrats) Scores	–.88*	.63*	.66*
Conservative Coalition Scores	.84*	–.41	–.74*
Presidential Support Scores	.85*	–.52*	–.75*
CCUS Scores	.75*	–.33	–.62*
Issue-Dimension I Scores	–.80*	.94*	.40
Issue-Dimension II Scores	.53*	.25	–.62*
Unique Contribution of Issue Dimensions	.12*	.14*	.04

Source: U.S. House Committee on Public Works roll-call votes (1973–1976).

*Statistically significant at the .01 level.

**Committee member during 90th Congress.

Republicans cannot arrive at a consensus either on regional allocation issues . . . or on traditional party issues" (1974, p. 169). Murphy notes that partisan divisions also occur on ideological issues: "Committee Democrats and Republicans cannot reach agreement on the authority of the Federal government or on its position in the economy" (1974, p. 169). Such disagreement may give rise to factions structured by both ideology and party. This may explain the small alignment of liberal Democrats in Faction 3, and the strong correlations between measures of ideology and partisanship among committee members. We therefore agree with Murphy that party is the most important influence on committee decision making, but we also note the influence of ideology on the alignments of committee Democrats.

Ways and Means Committee. Party and ideology define the divisions within the Ways and Means Committee. Faction 1 captures the ideological conflicts within the committee (Table 10). The lack of a distinct partisan complexion to this faction (1) and the strong bipolarity exhibited suggests an ideological organization to the faction: liberal Democrats tend to oppose conservative Democrats and Republicans. Partisanship structures the alignments of Factions 2 and 3—Democrats occupy Faction 2 and Republicans identify with Faction 3.

The correlates of the factional alignments are consistent with our definition of committee voting blocs: ideology and partisanship are highly correlated with each of the three factions. Factions 1 and 3 are ideologically conservative and tend to oppose the Democratic majorities. The partisanship reflected by Faction 3 is related to support for the policy

Table 10. Factions Within the House Ways and Means Committee

Committee Members	Factions F_1	 F_2	 F_3	h^2
**Phil M. Landrum (D-Georgia)	.87			.77
Omar Burleson (D-Texas)	.88			.84
Joe Waggonner, Jr. (D-Louisiana)	.88			.82
Bill Archer (R-Texas)	.77			.79
Donald D. Clancy (R-Ohio)	.63		(.46)	.63
John J. Duncan (R-Tennessee)	.60		(.45)	.56
**Charles A. Vanik (D-Ohio)	−.76			.78
James C. Corman (D-California)	−.63	(.47)		.61
Sam Gibbons (D-Florida)	−.60			.49
William J. Green (D-Pennsylvania)	−.64	.54		.76
Joseph E. Karth (D-Minnesota)	−.60	.56		.68
**Al Ullman (D-Oregon)		.83		.72
**James A. Burke (D-Massachusetts)		.51	(−.42)	.44
**Dan Rostenkowski (D-Illinois)		.81		.68
**Herman T. Schneebeli (R-Pennsylvania)			.79	.75
Barber B. Conable, Jr. (R-New York)			.87	.77

Correlates of Factional Alignments

	F_1	F_2	F_3
ADA Scores	−.80*	.80*	−.66*
COPE Scores	−.80*	.86*	−.84*
NFU Scores	−.84*	.87*	−.80*
ACA Scores	.89*	−.89*	.72*
Party Unity (Voting with Democrats) Scores	−.82*	.88*	−.81*
Conservative Coalition Scores	.89*	−.85*	.72*
Presidential Support Scores	.77*	−.81*	.87*
CCUS Scores	.90*	−.88*	.76*
Issue-Dimension I Scores	−.79*	.90*	−.87*
Issue-Dimension II Scores	−.61*	.22	.31
Unique Contribution of Issue Dimensions	.04*	.03	.03

Source: U.S. House Committee on Ways and Means roll-call votes (1973–1976).
*Statistically significant at the .01 level.
**Committee member during 89th Congress.

positions of the Republican administration. Faction 2 is more liberal and supportive of Democratic policy (Table 10–correlates). As in the other committees, the effects of ideology and party are intertwined; party unity scores are correlated above .9 with measures of ideology. Ideology and party, therefore, are important dimensions for the organization of the committee's factional structure.

Issues are relatively unimportant in describing committee factions (Table 10). In Faction 1, where issue dimensions significantly improve the explanation of the factional alignment, the contribution is a mere 4 percent. The issue dimensions may lack influence because the major dimension (I) is strongly correlated with partisan and ideological indicators. The impact of issues is probably captured by our measures of partisanship and ideology. The issue dimensions reflect positions on the taxing of oil and gas–Oil and Gas Energy Tax Act (I)–and on tax reform and energy conversion (II).

The ideological polarization within the committee–liberal Democrats voting in opposition to conservatives (Faction 1)–suggests that the nature of the House is an important influence on committee decision making. This is not an unexpected result since ideological cleavages within the House would be especially relevant to committees like Ways and Means and Education and Labor whose work has liberal-conservative dimensions. In Fenno's words,

> Of course, the very make-up of the parent chamber stands as a constraint on the activities of every committee. The changing size, for example, of "the conservative coalition" places outer limits on all committees whose work has liberal-conservative dimensions (1973, p. 22).

The partisan alignments in the committee's factional structure demonstrate the importance of party within the committee's environment. The impact of the administration on committee decision making may be obscured, however, by ideological and partisan environmental influences.

Our conclusions concerning the ideological and partisan factional structure of the Ways and Means Committee coincides with Fenno's characterization of committee decision making: "Ideological divergence parallels a strong set of partisan predispositions" (1973, p. 56). Thus, party and ideology continue to be important influences on committee decision making.

Summary and Conclusions

The similarity between our findings and those of previous studies may be a consequence of stability in committee membership, in goals of committee members, and in the influences operating on the committee's environment. In those committees where our findings differ from previous studies, we attribute the differences to the following factors: (1) the turnover in committee membership–Agriculture and Interior and Insular Affairs; (2) the change in the nature of the issues considered by the committee–Interior and Insular Affairs; (3) the collinearity between party and ideology–Interstate and Foreign Commerce; and (4) the alteration of environmental influences–Post Office and Civil Service.

We find that ideology is reflected in the factional alignments in the eight House committees. We attribute this to the salience of the House as an environmental influence on committee decision making. Further, this suggests that changes in the composition of the House–for example, greater ideological polarization–may alter committee decision-making practices.

Partisanship is an important explanatory variable in all of the committees except International Relations. This points to the important influence that the parties exert on the environment of House committees. We have suggested that partisanship may be augmented by the stage at which the votes were collected in some committees.

The salience of partisan and ideological elements in the voting patterns of House committees may only be temporary. For instance, clear partisan and ideological differences between Congress and the president during the period of this analysis may have sharpened the partisan and ideological factionalization within committee decision making. As these differences are muted by congressional and presidential elections, partisan and ideological differences between the members may begin to diminish. On the other hand, in light of the increasing complexity of issues, and especially amendments, ideology and party provide simplifying cues for voting on such issues. Increased work loads and less time to become familiar with complex issues may increase the role of such cues in voting.[14]

The effects of the administration are evident in two committees–International Relations and Post Office and Civil Service. This suggests that administration influences are of minor importance in the organization of committee factions; however, some of the effects of the administration may be muted by party, and to some extent, by ideology. As noted earlier, the votes in this analysis were taken during a period of partisan and ideological acrimony between the president and Congress. It may be that this situation augmented the partisan and ideological splits within the committee while concealing the influence of the administration. In fact, in the Post Office and Civil Service Committee, we suggest that increased administration interest in the issues it handled may have increased the amount of partisanship observed. Ways and Means is another example of a committee in which partisanship may be obscuring the effects of administration influences.

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[14] Since committee votes are less "visible" than floor roll calls, ideological voting may have more of an influence on committee roll-call behavior than on House floor votes.

In Agriculture, Education and Labor, International Relations, Post Office and Civil Service, and Public Works, variables other than ideology, partisanship, and administration are helpful in explaining factional alignments. In Education and Labor we find an ideological faction that also appears to reflect clientele influence (labor). Although their contribution is generally modest, issues do help to explain two of the factions in the Agriculture Committee and three of the factions in the International Relations Committee. In Post Office and Civil Service, issues reflect administration influences. Finally, issues dealing with mass transportation and the level of funding for such projects are important in explaining two of the factions in Public Works. As with the effects of the administration, some of the influence of issues may be hidden by ideology and partisanship.

In sum, we find that the House and the party, as reflected in ideological and partisan voting patterns, are the two dominant influences in the environment of the eight House committees studied. The dominance of these two factors may obscure some of the effects of administration and clientele influences. While knowledge of a committee's factional structure may not reveal the intricacies of committee decision-making practices, this information provides a baseline for mapping and monitoring changes in committee behavior. Such analyses may clarify voting relationships among committee members. Case studies of individual committees then can provide additional specification of committee decision-making patterns, and further delineate some of the influences which have been obscured by party and ideology. Finally, the type of analysis presented in this article may serve as a guide for other studies of committee decision making where interviews are difficult, if not impossible, to obtain.

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Relative Deprivation Revisited: A Response to Miller, Bolce, and Halligan*

FAYE CROSBY
Yale University

This article challenges some of the conclusions drawn in "The J-Curve Theory and the Black Urban Riots," by Abraham Miller, Louis Bolce and Mark Halligan (1977). Miller et al. reject relative deprivation theory and J-curve theory as valid explanations of black urban rioting. In my argument that Miller et al. are not justified in rejecting relative deprivation theory, I shall review four versions of relative deprivation theory to show how Miller et al. misrepresent the theory and to point out methodological problems with their operationalization of theoretical variables. Because these operationalization problems are far from atypical, I conclude with a call for greater methodological rigor.

Introduction

Recently an article by Abraham Miller, Louis Bolce, and Mark Halligan (1977) appeared in *The American Political Science Review* carrying the ambitious title: "The J-Curve Theory and the Black Urban Riots: An Empirical Test of Progressive Relative Deprivation Theory." In that article, Miller and his colleagues examine data collected biannually between 1956 and 1968 for four separate samples: northern whites, southern whites, northern blacks, and southern blacks. The data come from surveys apparently performed by the Institute for Social Research at the University of Michigan. On the basis of questions about income and education, Miller et al. construct three separate objective measures of outcomes. They also examine three separate subjective items: (a) one's perception of one's own financial trend; (b) one's expectations about future finances; and (c) one's "perception of his financial satisfaction" (p. 972). Basically, Miller et al. use their data to search for temporal patterns among the four samples and to discover the extent to which the six items intercorrelate. Their avowed primary objective is to test the J-curve theory as an explanation for the black urban riots.

On the basis of their empirical analysis and in the course of their methodological inquiries, the authors claim to discover several "primary" problems which "have consequences for the J-curve theory specifically and relative deprivation theory generally" (p. 965). The most important methodological problem, in the authors' view, is that relative deprivation researchers rely on aggregate-level data which are inappropriate to their concern with individual-level phenomena. The most serious conceptual problem, according to Miller et al., is that relative deprivation and J-curve theories assume a correspondence between current outcomes and future expectations, an assumption which they demonstrate empirically to be false. They then reject both J-curve theory and relative deprivation theory as valid explanations of the black urban unrest of the late 1960s. The authors conclude with the tentative suggestion that financial uncertainty among northern blacks may have led to the sense of frustration which culminated in the urban riots.

Purpose

My central purpose here is to demonstrate that Miller et al. are not justified in rejecting relative deprivation as an explanation of black urban unrest. Their analyses, I contend, suffer from two basic and interrelated problems. First, they misrepresent the relative deprivation literature. Second, they employ inadequate measures of felt deprivation.

This article is also intended to provide an antidote to the work of Miller et al. That they are overzealous in claiming to test relative deprivation theory may be a trivial point. More important in the long run are the false impressions which their article might have created. To help correct these false impressions, I shall review the relative deprivation literature in some detail, giving little attention to those aspects of Miller et al.'s article which do not pertain directly to relative deprivation.

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Why Dwell on Relative Deprivation? There are three reasons why I emphasize the issue of relative deprivation. First, Miller, Bolce, and Halligan emphasize it; testing relative deprivation theory appears to be one of the central purposes of the study. The term appears in the article's subtitle and in two of the internal headings. More than half of the paragraphs of the first two sections of their paper contain explicit references to relative deprivation. While Miller et al. specify in their title and in their opening paragraph that they are dealing with only one aspect of relative deprivation theory (i.e., progressive relative deprivation), they soon slip into the habit of referring to "relative deprivation theory generally," "most relative deprivation theories," or just plain, unqualified "relative deprivation theory."

More important is a second reason: relative deprivation is itself an important concept in the social sciences; it has strong interdisciplinary appeal. Since the term was coined by Samuel Stouffer and his colleagues during World War II (Stouffer, Suchman, DeVinney, Star, and Williams, 1949), relative deprivation has attracted the attention of political scientists, historians, sociologists, anthropologists, psychologists, and economists.[1] The area is one which continues to grow[2] and to inspire political controversy[3] after three decades of research.

The final reason to dwell on relative deprivation is that the article by Miller, Bolce, and Halligan may serve as a case study. Their article is representative of one major weakness in the relative deprivation research: looseness of operational definitions. Even when analyzing individual-level data, researchers have often been quite liberal in their translations from theoretical variables into operational measures. The deleterious consequences for attempts at "theory testing" are obvious.

Misrepresenting Relative Deprivation

Miller, Bolce and Halligan misrepresent both the theoretical and the empirical work on the subject of relative deprivation. They fail to define relative deprivation, mistakenly equate relative deprivation theory and the J-curve theory of revolution, ignore three major relative deprivation theorists and present a slanted view of the major tenets of the fourth. They make a false claim about the theory of relative deprivation and also make false claims about the nature of the empirical research. Let us examine each of these problems in turn.

Lack of Definition. One obvious flaw in the article which purports "to test relative deprivation theory in general" is the absence of an explicit and succinct definition of the term "relative deprivation." During the last two decades, relative deprivation has been used in two general senses in sociological, psychological and political science literature. First, the term "relative deprivation" is used to refer to the *emotion* of deprivation, an emotion generally conceived of as one type of anger roughly synonymous with a feeling of resentment or dissatisfaction or with a sense of grievance. Second, the *theory* of relative deprivation refers generally to the proposition that the emotion of deprivation is not simply a function of an individual's objective status. The theory of relative deprivation attempts to specify the conditions under which objective (absolute) deprivation eventuates in subjective (relative) deprivation and to describe the ways in which felt deprivation results in various behaviors (e.g., violence, apathy).

Equating J-Curve Theory and Relative Deprivation Theory. How closely J-curve theory corresponds to relative deprivation theory is a matter of some debate (Geschwender, 1964; Cook, Crosby, and Hennigan, 1977). In his two best-known articles, Davies (1962, 1969) makes no explicit reference to the concept of relative deprivation. On the other hand, Gurr (1970), one of the major relative deprivation theorists, states in no uncertain terms that Davies' J-curve illustrates well what he, Gurr, intends by the term "progressive deprivation." Certainly, in view of Gurr's opinion, Miller and his colleagues seem justified in considering the J-curve theory as one specific form of relative deprivation theory.

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[1] Political scientists interested in relative deprivation include Nagel (1974) and Gurr (1968, 1970). Sociologists include Davis (1959), Geschwender (1964), Merton and Rossi (1957), Runciman (1966), and Useem (1975). Historians McFarlane (1970, p. 150), and Shorter and Tilly (1974) also mention relative deprivation. Among the psychologists are Adams (1965), Brickman and Campbell (1971), Crosby (1976), and Pettigrew (1964, 1967). Economists interested in relative deprivation include Hibbs (1974) and Hirschman (1973).

This list represents only a sample of scholars who have been concerned with relative deprivation. For a more complete list, see Crosby (1976). Pettigrew's work merits special attention in the present context: Pettigrew was the first to discuss how actual gains may have led to psychological losses among northern black Americans.

[2] Recent articles include Butler (1976), Cook, Crosby, and Hennigan (1977), and Williams (1976). Recent doctoral dissertations are Hennigan (1977) and Martin (1978).

[3] Some, like J. Sawyer (1971), have criticized the theory for its repressive stance; others, like Mednick, Tangri, and Hoffman (1975), have praised it for its radicalism.

Arguing that J-curve theory may be subsumed under the more general rubric of relative deprivation is one matter. It is quite another matter to argue that because J-curve theory is invalid, all of relative deprivation theory is invalid. Miller et al. take this line of reasoning more and more as their article unwinds until, in their summary, they refer to "relative deprivation-based theories" as if there were no difference between one quite specific pattern of relative deprivation identified by Davies and Gurr and all other formulations.

Omission of Three Versions of the Theory. Miller, Bolce, and Halligan misrepresent the theory of relative deprivation by acknowledging only one of the four standard models of relative deprivation. The authors do mention the political scientist Ted Robert Gurr, but they ignore the sociologists, Davis (1959) and Runciman (1966), and the psychologist Crosby (1976). My own contribution (Crosby, 1976) is probably too recent to have come to the attention of Miller et al.; however, it is difficult to understand how Davis and Runciman could have escaped their notice, given that Davis' article is nearly two decades old, and that Runciman's work is discussed at length by Gurr. Let us summarize here the theories of Davis, Runciman and Crosby with a view to asking: can the measures used by Miller et al. test any of these theoretical formulations?

Davis' (1959) model of relative deprivation represents the first attempt to make a formal theoretical model about the phenomenon identified by Stouffer and his colleagues (1949). Davis makes two central distinctions. He distinguishes between in-group members and out-group members and between "haves" and "have-nots." When a person (*P*) compares himself[4] with some other (*O*) of his own in-group, if *P* is a "have-not" while *O* is a "have," deprivation is experienced. In contrast, comparisons with better-off out-group members, according to Davis, create or reinforce social distance. Davis further postulates that comparisons are made at random within the in-group. The probability of in-group members experiencing deprivation is the joint probability of each member being deprived himself *and* of each deprived member comparing himself to another better off. It follows, mathematically, that the probability of relative deprivation is a curvilinear function of the proportion of "haves" and "have-nots" with the likelihood of felt deprivation being greatest when the in-group is evenly split between "haves" and "have-nots."

The most direct test of Davis' model of relative deprivation would be to ask respondents how they felt about certain comparisons (e.g., making within-group comparison to a better-off other).[5] An indirect test, using aggregate data, would be to determine whether unrest increases as the economic differences between distinct social groups diminish.[6] The analyses which Miller et al. perform obviously do not constitute a test of Davis' model. Nor do their analyses advance our understanding of how blacks come to view whites as members of their own in-group in Davis' terms. As Sears and McConahay (1970), Caplan (1973) and Paige (1968) have noted, shifts in the definition of whites as in-group or out-group may have contributed to black urban unrest.

More widely recognized than Davis' theory is Runciman's (1966) theory of relative deprivation. Runciman distinguishes between egoistical, fraternal, and double deprivation. Egoistical deprivation occurs when individuals feel personally deprived relative to others in their comparison group. Fraternal deprivation occurs when individuals feel that their own group is deprived relative to another group, although they are personally gratified. Double deprivation occurs when individuals feel both egoistically and fraternally deprived. Runciman also discusses the fact that people can experience status and power deprivation as well as economic grievances. He specifies that deprivation will be felt when individuals who lack some desired object or opportunity (which we shall call X) (1) want X; (2) compare themselves with better-off others (including themselves in the past); (3) feel they deserve X; and (4) think it feasible to attain X (Runciman, 1966, p. 11).

Miller et al. clearly do not speak to Runciman's version of relative deprivation theory. First, their data provide no basis for a validational test of egoistical deprivation or of fraternal deprivation among blacks. Second, they ignore the status and power dimensions. Finally, it is hard to make inferences about the preconditions of felt deprivation specified by Runciman on the basis of their data.

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[4] The pronouns *he, him,* and *himself* are used in the generic sense. Masculine pronouns are used for succinctness and refer to both females and males.

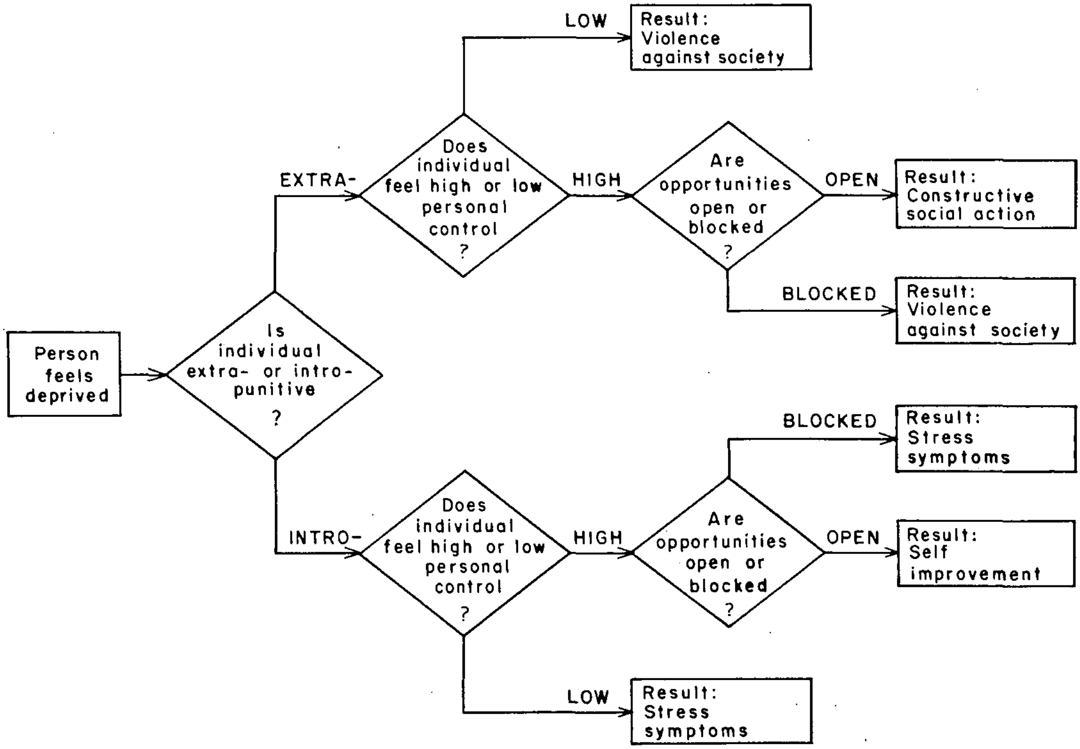
[5] Patchen (1961) has done just this.

[6] This approach is seen in Lieberson and Silverman (1965) and Nagel (1974).

My own model of egoistical relative deprivation (Crosby, 1976) features felt deprivation as part of a chain of variables including (1) environmental antecedents, (2) preconditions, (3) felt deprivation, (4) mediators, and (5) behaviors. Building on the work of Runciman (1966) and Pettigrew (1967), I posit that an individual will experience deprivation when and only when the four preconditions specified by Runciman are joined by a fifth: lack of personal responsibility for current failure to possess X. When one blames oneself for failure to possess X, one does not experience deprivation. I claim that the relationship between the preconditions of deprivation, on the one hand, and felt deprivation, on the other, are at the core of the model. Surrounding that core are other relationships. The links between various environmental antecedents (e.g., percentage of others possessing X) and each of the preconditions of deprivation are discussed. Perhaps of more interest to students of black urban unrest is my analysis of how the emotion of deprivation results in one of four behaviors. Figure 1 depicts the way in which the emotion-behavior relationship might be mediated by three other variables, intro-extrapunitiveness, personal control, and actual opportunities.

Obviously, one could not test my model on the basis of the data presented by Miller and his colleagues. Even if one were willing to hazard a guess about the likelihood of felt deprivation occurring among each of their four groups over time, one would have no way of gauging the level of personal control or of extrapunitiveness among the sample of northern blacks.

Partial Coverage of Gurr. Gurr (1968, 1970) is the only one of the four basic relative deprivation theorists who seems to have come to the attention of Miller, Bolce, and Halligan. Even so, Gurr's version of the theory is not adequate-


Person feels deprived
Is individual extra- or intro-punitive ?
EXTRA-
INTRO-
Does individual feel high or low personal control ?
LOW
Result: Violence against society
HIGH
Are opportunities open or blocked ?
OPEN
Result: Constructive social action
BLOCKED
Result: Violence against society
Does individual feel high or low personal control ?
HIGH
Are opportunities open or blocked ?
BLOCKED
Result: Stress symptoms
OPEN
Result: Self improvement
LOW
Result: Stress symptoms

Source: Adapted from Faye Crosby (1976). "A Model of Egoistical Relative Deprivation." *Psychological Review* 83:100.

Figure 1. Felt Deprivation, Mediating Variables, and Behavioral Outcomes

ly represented. Gurr posits that relative deprivation is the anger or distress that results from a discrepancy between "should" and "is." More formally, his central proposition is:

$$RD = \frac{Ve - Vc}{Ve}$$

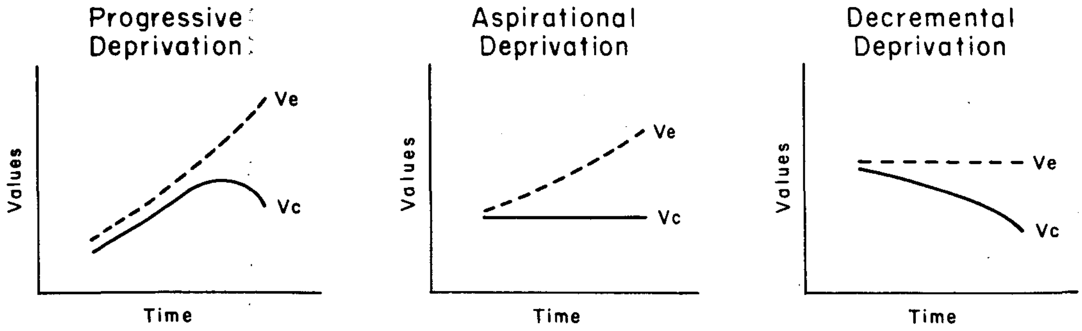
where *RD* stands for "relative deprivation," *Ve* stands for "value expectations," and *Vc* stands for "value capabilities." Value expectations are the goods and opportunities which people want and to which they feel entitled; value capabilities are the goods and opportunities which they have or think it feasible to attain.

Gurr identifies three patterns of deprivation: aspirational, decremental, and progressive. Aspirational deprivation occurs when value capabilities remain constant over time while value expectations increase. Decremental deprivation occurs when value capabilities decrease over time while value expectations remain constant. In progressive deprivation, value capabilities decrease while value expectations increase. The three patterns identified by Gurr are shown in Figure 2. Miller et al., as the title of their article identicates, limit themselves to the last pattern: progressive relative deprivation. Certainly they cannot be criticized for this. Much more open to criticism, however, is their tendency to refer to "Gurr's theory" or to "relative deprivation theory in general" when they are only, in fact, alluding to one pattern of deprivation identified by one of the theorists.

False Claim about Theory. Miller, Bolce, and Halligan reject relative deprivation as an explanation of their data, in part, because their data show no correspondence between present and future perceptions and because Miller et al. claim that relative deprivation theory demands such a connection. They refer, for instance, "to the untested assumption of relative deprivation theory generally and the J-curve specifically that one's current trend of need fulfillment is positively and strongly related to future financial expectations" (p. 973). Miller et al. reinforce their claim, alluding to "the all-important assumption of relative deprivation theory generally that current need fulfillment is related to expectations of future need fulfillment" (p. 976). They declare that "this assumption [is] virtually axiomatic to all forms of relative deprivation theory" (p. 977).

Miller et al. then concentrate on demonstrating that the supposedly "all-important assumption" is false. To accomplish this, they (a) derive four hypotheses which would follow from the assumption and (b) marshal empirical evidence to reject the hypotheses. They then rest their case against the J-curve theory in particular and "relative deprivation generally" as explanations for the black urban riots. The authors do a creditable job of demonstrating that the derived hypotheses are incorrect. Their case is unconvincing, however, because the hypotheses are themselves derived from a false claim.

In plain and simple terms, relative deprivation theory does *not* assume a correspondence between present conditions and future expectations. Indeed, the proposition that expectations are not simply a function of present status is central to relative deprivation theorizing in general. More specifically, Davis' model of relative deprivation makes no reference at all to



Source: Adapted from Ted Robert Gurr (1970). *Why Men Rebel.* Princeton: Princeton University Press, pp. 47, 51, 53.

Figure 2. Gurr's Three Patterns of Relative Deprivation

the notion of future expectations. Both Runciman's and my own theories include future expectations. According to Runciman and Crosby, one cannot feel deprived about the failure to possess X unless one perceives that it will be possible to attain X. The source of future expectations may be past outcomes or the present outcomes of one's neighbors or, in fact, a variety of stimuli *other than one's own current outcomes.*[7] Nor is the link between current and expected states assumed by Gurr. In fact, at the very basis of Gurr's models of aspirational deprivation and decremental deprivation is the axiom that future expectations are not derived only from current outcomes.

Empirical Literature. Miller, Bolce and Halligan present a biased general picture of the pertinent empirical literature. They claim that "relative deprivation theorists generally . . . perform their empirical testing with aggregate social and economic indicators" (p. 968) and chide the researchers for a "reliance on objective data" (p. 969). The charge applies with force to the archival studies of cross-national patterns of violence (e.g., Gurr, 1968, 1970; Feierabend and Feierabend, 1967). These studies do not apply to the literature which examines black unrest in America in the context of relative deprivation and social comparison theories. The majority of empirical studies in black unrest involve cross-sectional survey data of much the same variety as those analyzed by Miller et al. The surveys, furthermore, obtain subjective as well as objective measures. Of the 20 studies of black unrest reviewed by Crosby (1976), for instance, only four relied solely on objective data.

The coverage of the empirical literature is flawed in its particulars as well as in its general aspects. One example may suffice. Miller et al. cite the well-known article by Clark McPhail (1971) which argues against relative deprivation as an adequate explanation of the black urban riots. They state that "the studies cited by McPhail are generally based on analyses of socioeconomic and political characteristics of riot versus non-riot cities" (p. 966). This is false; the studies cited and analyzed by McPhail are surveys. They concern the characteristics of riot participants versus nonparticipants. The data are individual-level, not aggregate-level.

Operationalization Problems

The General Problem. Although the relative deprivation empirical literature does not suffer from the ills which Miller et al. diagnose, the literature is hardly symptom-free. More specifically, the empirical literature appears to be infected by a lack of correspondence between operational definitions and theoretical definitions. Leaving aside the issue of aggregate data, most of the supposed measures of relative deprivation lack "face validity." In other words, the specific indicators do not appear (on the face of it) to measure the theoretical variables for which they are named.

Gurr's work may serve to illustrate the point. Gurr, one may remember, defines deprivation as the discrepancy between "is" and "ought" and claims that the amount of felt deprivation is theoretically equal to the sum obtained by subtracting *Vc* from *Ve* and dividing the difference by *Ve*. So much for theory. In practice, Gurr distinguishes between persisting deprivation and short-term deprivation. His score for the former is obtained generally by calculating the sum of six measures, weighted for importance. These include (a) economic discrimination (exclusion of some groups from economic arena), (b) political discrimination (exclusion of groups), (c) potential separatism (size of separatist regions or groups), (d) dependence on private foreign capital, (e) religious cleavages, and (f) lack of educational opportunity. What, one wonders, do these measures have to do with the discrepancy between ought and is? One can, for instance, easily argue that economic, political, and educational discrimination all serve to decrease and *not* to increase felt deprivation. If consistently applied, such sanctions ought to create contentment, not contention, among the objectively deprived in a nonegalitarian system. The methodological purist might wonder why Gurr, who spends considerable time defending the use of aggregate measures, seems mute when it comes to defending the appropriateness of his specific operational definitions.

Gurr and the other researchers who use archival data are not the only ones to violate the requirements of face validity. Runciman, for example, fails to obtain measures of resentment or to ask respondents about their feelings of grievance even though he regards feelings of resentment or grievance as synonymous with felt deprivation. He does ask his respondents how satisfied they feel, and dissatisfaction may be assumed to vary with deprivation. He also asks them whether they believe that anyone is better off than they themselves (comparison other) and, if so, how they feel about this situation. Ironically, Runciman does not make much of these data even though they are among the most direct measures of felt deprivation collected in surveys to date.

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[7] Both Runciman and Crosby are explicit on this point. Crosby is perhaps more systematic than Runciman. See especially Table 2 in Crosby (1976, p. 93).

Particular Case. As indicated earlier, Miller, Bolce, and Halligan examine three objective and three subjective measures in their research. While they acknowledge that the objective indicators are not good measures of relative deprivation, they assume that the three subjective measures are. This assumption requires closer scrutiny.

Miller et al. refer to the three subjective measures as "perceptual indicators" and describe them thus:

> These items are: (1) whether the respondent (R) perceived his financial situation as being better or worse than the year before; (2) R's expectation of his future financial prospects; and (3) R's perception of his financial satisfaction (p. 972).

Of the three indicators, financial satisfaction may approximate felt deprivation while perceived trends and future expectations have little or no face validity. The inadequacy of Miller et al.'s measures of felt deprivation becomes obvious if we return to the four basic versions of relative deprivation theory.

All four theorists conceive of relative deprivation as an emotion and not simply as a perception.[8] Davis refers to felt deprivation, not to perceived deprivation. Runciman is more explicit: he distinguishes between the perception of unequal outcomes and the emotional reaction to that perception. The latter, and not simply the former, is the equivalent of relative deprivation. Similarly, Crosby explicitly defines the concept of relative deprivation as an emotion; felt deprivation in this model is one type of anger and is synonymous with a feeling of resentment or a sense of grievance. For Davis, Runciman, and Crosby, the third "perceptual" measure used by Miller et al. clearly provides an approximate measure of or indirect measure of felt deprivation. Miller et al.'s other two perceptual indicators are not relevant for Davis' formulation; for Runciman's and Crosby's formulations, they are clearly indicators of one of the hypothesized preconditions of felt deprivation but not of felt deprivation itself.

Miller, Bolce and Halligan's perceptual measures bear a complex relationship to Gurr's version of relative deprivation theory. Gurr proclaims that felt deprivation is roughly synonymous with dissatisfaction; thus, their third measure appears to be a valid indicator of deprivation in Gurr's model. Gurr also says, however, that value capabilities include not only one's present level of outcomes but also one's expected level. Thus, it would seem that the best approximation of felt deprivation for any of the four samples during any of the time periods would be the percentage of respondents who (a) are currently dissatisfied *and* (b) expect no improvement. In their present form, then, Miller et al.'s "perceptual indicators" do not adequately operationalize even Gurr's model of relative deprivation.

Given that Miller et al. may be more interested in testing Davies' J-curve hypothesis than in testing relative deprivation theory, it seems only fair to consider how well the perceptual indicators represent Davies. Two points are important here. First, most of the testing of Davies has to do with the objective measures. Davies does indeed refer to "states of mind" but his theory deals primarily with an observable pattern of objective measures. One may say that Davies posits a certain pattern of objective data and speculates about the subjective meaning of the pattern. Second, to the extent that Davies does speculate about inner states, he centers his observations around the proposition that "the crucial factor [in precipitating revolution] is the vague or specific fear that ground gained over a long period of time will be quickly lost" (Davies, 1962, p. 6). In view of this proposition, Miller et al. might create a combined perceptual indicator of "revolution proneness" among the four samples over time by reporting the percentage of respondents who (a) see the current year as better than the year before *and* (b) expect a deterioration of conditions in the future. Such a measure would not provide the optimum operationalization of Davies' theoretical speculations, but it seems superior to any one of the perceptual indicators considered alone.

Small Critique

As indicated earlier, this article is not a point-by-point critique of Miller et al.'s essay. For the sake of completeness, however, I shall briefly mention some of the other faults in the J-curve article and also some of the article's strengths.

Problems. In addition to the major problems already noted, Miller et al.'s article contains four interrelated weaknesses. First, the authors convey very little information about their sample. It is obvious from Tables 6, 7, and 8 in their article that the samples differed greatly in size, with the northern white sample being disproportionately large. Were all the samples selected in exactly the same way? Were the respondents all in one national probability sample or was stratified random sampling used? More details would reassure the reader that the four data sets were, in fact, comparable. Second, one wonders why Miller et al. group their data by regions (North, South) rather than by riot areas. They themselves argue against the view of society-as-monolith. Why, then, treat either the North or the South as a monolith?

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[8] For further discussion of this point, see Cook et al. (1977, pp. 308–10).

Closely related to the second weakness is a third: Miller et al. make the point that aggregate-level data display a different pattern from individual-level data. The support for this point comes primarily from the contrast between Table 1 and Figure 2 on the one hand, and Table 2 and Figure 3 on the other. Unfortunately, the former pair contain Davies' data while the latter pair contain the Michigan election survey data. If Miller et al. had *aggregated their own data,* would the resultant pattern have resembled Davies' aggregated data or their own individual-level data? One wonders.

The final weakness in Miller, Bolce, and Halligan's work relates to a statistical issue. The authors observe that the perceptual data fluctuate more among the northern black sample than among any of the other three samples. They then tentatively propose that financial uncertainty may have been at the root of the frustration which presumably characterized the northern blacks. Given the vast differences in sample size, it is possible that the apparently great differences in the temporal patterns characteristic of the four samples were no greater than might be expected on the basis of chance alone. In fact, when the data presented by Miller et al. in their Tables 6, 7, and 8 are re-scored and analyzed for heterogeneity of variance, the uncertainty fluctuation hypothesis is only weakly supported.[9]

Strengths. Miller et al.'s article is, obviously, not without merit. First, their concern with appropriateness of various types of data is a strong point. The idea of comparing individual-level and aggregate-level data is quite good indeed. Their attempt to intercorrelate the various objective and perceptual measures of deprivation also deserves praise. Fourth, Miller, Bolce and Halligan are clever to think of testing Davies on his own ground. Finally, one ought to commend the authors for their observation that relative deprivation may appear to explain more than it actually explains. Relative deprivation has been applied as a post-hoc explanation of events, but its predictive powers are still largely untried. Perhaps as a result of this fact, the concept has been employed without great precision.

Conclusion

Miller, Bolce and Halligan claim to disconfirm relative deprivation theory and the J-curve theory as valid explanations of the black urban riots. Their claim, we have argued, is based on a misrepresentation of the relative deprivation literature and on inappropriate operational measures. The data presented and analyzed by Miller and his colleagues provide no basis for testing relative deprivation. We have tried here to present an informed picture of the relative deprivation literature and have raised some methodological issues as a corrective to Miller et al.'s view.

Devising measures and obtaining data which would more adequately test any one of the relative deprivation theories is not an easy task. The difficulty of the task may at times make one wonder if the candle be worth the game. On the basis of the very large number of empirical findings which corroborate aspects of relative deprivation theory, however, we suspect that attempts to validate (or invalidate) relative deprivation theory are worth doing and worth doing well. Fast and loose "tests" of relative deprivation have probably outlived their utility; the time for methodological rigor has arrived. As Cook, Crosby, and Hennigan note in a recent methodological critique of relative deprivation:

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[9] Morty Bernstein and I analyzed the data presented by Miller, Bolce and Halligan in Tables 6, 7, 8 (Miller et al., 1977, pp. 975, 977, 978). Table 6 is labeled "R's Perception of the Trend of his Financial Situation by Race and Region." We considered that each "sample" (e.g., northern white) had seven data points (1956, 1958, 1960, 1962, 1964, 1966, 1968). For each data point, we computed a score by assigning 2 to the category "better," 1 to "same" and 0 to "worse." Thus, for example, the northern white score for 1956 was 121. We then compared the northern black condition with the northern white to determine if there were significant heterogeneity of variance. For Table 6 the results were statistically nonsignificant ($p > .05$); for Table 7 they were marginally significant ($.01 < p < .05$). For Table 8 they were significant ($p < .01$).

> relative deprivation has enjoyed thirty years of usage in the Social Sciences. This long period makes it difficult to argue that the construct is exciting and new and deserves to be used loosely in order to achieve a first sense of its explanatory potential.... This point of view may have made sense two decades ago, but not today (Cook et al., 1977, p. 330).

Cook et al. direct their comments primarily to the overly zealous proponents of relative deprivation, but these comments apply with equal force to the critics.

Editor's Note: Abraham Miller and Louis H. Bolce will respond to this article in a future issue of the *American Political Science Review.*

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Aristippus in and out of Athens

STEPHEN TAYLOR HOLMES
Harvard University

This article has two purposes. The first *is to show how some of the central principles of classical Greek political theory became anachronistic as a result of massive transformations in the underlying structure of European society. These principles, it is argued, were originally dependent on an empirical premise that the polity is a "whole" encompassing individual "parts," or (stated differently) that the polity is identical with total society. This whole/part schematization of the polity seemed plausible in the ancient city since most sectors of* polis *life had political connotations or overtones. The same schema, however, became an archaism in modern Europe, chiefly because of the emphatic emergence of a distinction between state and society—one aspect of a more general increase in the structural differentiation of society. The* second *and closely related purpose is to explore the feasibility of a claim once advanced by Benjamin Constant: that the organizational transformations involved in the modernization of European society have created a novel rhetorical opportunity, the possibility of defending tyranny in the name of freedom and democracy.*

I

Surprisingly enough, numerous assumptions underlying contemporary, including neo-Marxist approaches to the problem of political legitimacy are still essentially Greek. For this reason they are also essentially anachronistic. Almost without our noticing it, they have been worn out by time. More precisely, their contemporary relevance has been subtly and irrevocably undermined by the gradual shift first to the medieval and eventually to the modern type of European society. In this article I shall focus on two—now obsolete, though still enticing—premises of Greek political thinking: (1) that the state can be "humanized" as a dialogue, family or emotional communion with a "true" and therefore unifying purpose, and (2) that individuals, being thoroughly "political animals," can fully realize themselves in political participation. Not utterly implausible for the *polis,* it will be argued, both ideas lead to personal and governmental deformations when "revived" in highly differentiated and rapidly changing modern societies.[1]

Indeed, the Greek claim that all spheres of life should be subordinated to politics cannot be translated into a modern European language without divulging its contemporary irrationality. For us, such a claim seems to stand or fall with the patently absurd assumption that politics (and the absurdity stems from what "politics" now implies) can solve all human problems, indeed that it can bear the entire burden of making people happy, free and in touch with themselves. Although this promise may have made some sense in a small city-state or *Gemeinschaft,* it is clearly out of step with the structural realities of a large modern society or *Gesellschaft.* Nevertheless, its stubbornly persistent influence on and attractiveness for much contemporary political theory can hardly be denied.

Verbally, of course, it is possible to defend the "timeless validity" of the works of Plato and Aristotle.[2] For instance, it may seem decisive that both ground political "legitimacy"[3] in forms of "participation" and "representation." Yet the prima facie modernity of these ancient ideas is deceptive. As a matter of fact, the basic moral claims advanced in the political writings of Plato and Aristotle are inseparably linked to descriptive postulates about the institutional framework of the city-state. As might be expected, the *polis*-bound connotations of these claims are irreconcilable with the structural realities of political participation, representation and legitimacy as they began to stabilize themselves in eighteenth-century democratizing bourgeois contexts, and as they continue to exist, precariously to be sure, in constitutional-pluralist societies today.

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[1] For one of the earliest and most provocative accounts of how modern attempts to resuscitate ancient liberty unavoidably result in tyranny, see Constant (1957, Pt. 2, Chs. 6–8). Constant's basic insight, which I want to pursue in this article, is that the old *res publica* conception of politics, renewed in modern times, only serves to overlegitimate a technically efficient bureaucratic agency with police powers. The idea that only politics provides a "public space" for human self-realization or (what amounts to the same thing) that politics is "man's essence" makes it nearly impossible to understand why citizens might want to resist the coercive encroachments of a hypertrophic state.

[2] No Marxist would openly defend such an "unhistorical" belief. Yet its conservative defenders (and I am thinking here of Leo Strauss and his followers) reveal a kind of "inverted agreement" with the most brashly totalitarian of the leftists. Telling in this regard is Strauss' exchange with Alexandre Kojève (Strauss, 1968a). Their chief point of consensus, so it seems, is the "alienating" character of post-Copernican and capitalist modernity where human beings have all purportedly become Benthamite atoms (instrumentalizing each other) rather than civic collaborators (harmonizing with each other).

Not unpredictably, the indiscriminate use of a single set of concepts to elucidate such widely divergent situations as the ancient and the modern has produced a disorienting lack of focus in the philosophy of politics. It is my conviction that a cautious appeal to the idea of modernization developed by the pioneer social theorists of the late nineteenth and early twentieth centuries can help us correct the conceptual displacement at the root of this trouble. On the basis of a theory of social evolution borrowed selectively from Durkheim, Simmel and Weber,[4] it is possible (I believe) to make a strong case for the following thesis: the totalitarian insistence that government should be everywhere and that politics can solve all of humanity's fundamental problems is *both* alluring *and* irrational because it anachronistically echoes the Greek identification of the political sphere with total society.

In defending this thesis, I will first explain how my argument can be understood as a response to the three writers whose works have dominated the postwar discussion of Greek politics and political theory. This will allow me to suggest, in a preliminary fashion, how a general theory of social evolution can provide descriptive orientation for normative discussions of political principles (part II). I will then go on to state, in cursory fashion, what I take to be Aristotle's (and to a lesser extent Plato's) fundamental ideas about political participation and representation. The ethical doctrines found in the classical texts of Greek political philosophy, or so I try to show, are inextricably bound to the empirical premise that society can be adequately grasped as a "whole" made out of "parts" (III). I next examine some of the arguments advanced by Aristotle in support of his notorious principles that the city is "prior" to the citizen and that man is an essentially "political animal" (IV), recalling briefly some of the special characteristics of the classical *polis* which lent a degree of everyday plausibility to these seemingly abstruse philosophical claims (V and VI). Finally, I suggest the precise way in which, due to a special set of institutional transformations in the West since the end of antiquity and especially since the inception of the modern or "bourgeois" age, this whole/part schematization of society has become obsolete (VII). As a direct consequence of this gradual yet dramatic structural change, or so it will be argued, the principles of Greek politics become flagrant and despotic archaisms when transported, even with the best of intentions, into the institutional context of modern society (VIII).

II

Paradoxically, the inherent difficulty of acquiring a satisfying overview of Greek politics and political philosophy, as well as of their relevance today, seems to have been exacerbated by the work of three brilliantly controversial (and distinctly non-Marxist) exiles from Hitler's Reich. To be sure, there are vast discrepancies of tone, purpose, and political commitment separating Karl Popper's *The Open Society and its Enemies* (1966), Leo Strauss' *Natural Right and History* (1953), *The City and Man* (1964) and *On Tyranny* (1968), and Hannah Arendt's *The Human Condition* (1958a). Simplifying slightly, we can formulate the basic disagreement in the following way: while Popper views totalitarianism as a *return* to Plato and Aristotle, Strauss and Arendt view it as the grim consequence of having *turned away* from the two greatest philosophers of classical antiquity. Underlying this formidable conflict of opinion, moreover, is a profound disagreement about the contribution of modern "scientific" rationality to contemporary social and political life. But these startling differences only emerge so sharply because of a background of shared and unswerving belief. Each of these books was written in the conviction that the study of Greek theories concerning law, politics and society, far from being dusty antiquarianism, is of decisive importance to those facing the tragedies and troubles of twentieth-century civilization. Popper, Strauss, and Arendt (and given the European experience it is easy to see why) all agree that the fundamental political problem of our age is totalitarian autocracy. And their shared concern for the Greeks is motivated by the compelling need to evolve an adequate understanding of the threat to the democratic West posed by the total state.

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[3] The fact that Greek has no precise counterparts to the elementary terms of modern political philosophy should reinforce the point I am trying to make. The Greek terms which perhaps come closest to "legitimate" (as characterizing a justified regime) are *orthos*, right, true, straight or correct, and *kata phusin*, according to nature. Clearly, the modern claim is quite moderate, even "minimalist," in comparison to the Greek. The same will appear to be the case when we turn below to the ostensible Greek analogues of "participation" and "representation."

[4] Of course, the best-known recent endeavors to elaborate on the classic sociologists' claim that increasing structural differentiation is the fundamental characteristic of modern society have come from Parsons and his school. (See, for example, Parsons, 1966, 1971.) For an extremely helpful attempt to reformulate and expand Parsons' theory of social differentiation while abandoning his Durkheimian insistence that all societies require "normative integration," see the numerous (and in Germany, at least, widely debated) works of Niklas Luhmann. For a programmatic statement of his position, see Luhmann (1972, 1975).

To these common concerns most of us would not object. To a large extent we share them. The dramatically clashing conclusions, however, should serve to make us wonder. In reconsidering the basic claims found in the books listed above, the nonpartisan reader is struck by their authors' common and unmitigated indifference to that tradition of empirically oriented social theory which has contributed so much to our understanding of the *specifically modern conditions* under which Greek philosophy has become archaic and totalitarianism has become the threat to humanity it is. The methodological premise underlying my own argument is as follows: the difficult and constantly recurring problem of the relation between Greek political theory and the modern total state can only be clarified if placed within the context of a theory of the structural and institutional evolution of Western society. As I will try to explain below, the theories which I consider most helpful in this regard tend to interpret social evolution primarily as a process of increasing structural differentiation. All three of the authors in question have systematically neglected theories of this type.

Take Popper, for example. His exuberant indictment of Plato as a totalitarian and protofascist is devastating but unconvincing. Popper's critics, however, have gone astray in struggling to exonerate Plato from the charge that he favored a closed, collectivist and regimented state. The glaring weakness of *The Open Society and its Enemies* is not Popper's characterization of Plato (though this has its flaws), but rather his strange assumption that modern liberalism could have been anticipated by Lycophron, Alcidamas and others to whom we find it solemnly attributed. Plato was doubtless more strongly attached to the *stratopedou politeia* or "garrison state" than certain prodemocratic Athenians. But there is no evidence that even the most vehemently anti-Spartan party was "liberal" in the modern sense.[5] Modern liberalism, as I will suggest in a moment, could only arise after the prior emergence of a theologically interpreted religion, a completely monetarized economy, and a political sphere robbed by nonteleological science of any chance to gain legitimation by an appeal to "natural purposes." As a consequence, liberalism could not even have been articulated as a coherent position in classical Greece, in a society with both a civic religion and an economy submerged in a surrounding nexus of political and military norms, and where there was a widespread belief in some kind of "natural law" capable of lending moral legitimacy to the regime. As a consequence, and in spite of all the Popperian efforts of Eric Havelock, ancient advocates of full-scale participatory democracy cannot be considered "liberal" in any meaningful modern sense.[6]

The elementary characteristic which Simmel, Durkheim, Weber and their more recent followers have attributed to the modern social order is a high degree of social differentiation or the growing autonomy (and, hence, roundabout interdependence) of a number of distinct interaction contexts or channels of communication such as religion, science, art, law, economy, politics, education, and intimate family life.[7] I will return to this—I believe—decisive analysis below. For now, let it suffice to recall the following: in antiquity the only rigorously defined cleavage (besides those of class) commonplace in discussions of the structure of society seems to have been the breach between publicity and privacy, *ta koina* and *ta idia*, the agora of discussion and the agora of exchange. Roman epicureans were referring to a similar split when they distinguished the *res publica* from the *hortus conclusus*. In modern society, on the other hand, it would be ludicrous to suggest that whenever people withdraw from political activity they must (by definition!) sink into "solitude" or egoistic self-seeking. The old binary schema public/private, in fact, is simply too crude to help us understand ourselves. There are (and quite explicitly in the West) plenty of nonpolitical avenues of *social* interchange, and we take advantage of them every day. It is, in fact, the rather improbable evolutionary emergence of a pluralistic and "centerless" *communicative* network which lends plausibility to the modern liberal notion of "negative freedom," and especially to the idea that human beings should be free not to participate in politics. For instance, the right to *publish* scientific hypotheses, emphasized by Mill, is obviously not meant to shelter Epictetus' *interior domus*.[8] Its value lies rather in its capacity to help preserve *a channel of social communication* from repressive politicization and control.

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[5] It should be mentioned here that in the preface to the second edition, written seven years after the original manuscript was completed, Popper refers to liberalism as "a movement which began three centuries ago," thus contradicting the central thesis of his book (Popper, 1966, p. ix).

[6] Havelock's quaint and rambling treatise (1957) suffers from the same defect as Popper's study on which it is based. Neither author betrays any awareness of the decisive linkage between the modern liberal doctrine of the "bourgeois rights of man" and the high degree of structural differentiation unique to modern bourgeois society. For Strauss' criticism of Havelock, see Strauss, 1968b, pp. 26–64.

[7] For the sake of synopsis, it is possible to locate—following Weber, Parsons, and Luhmann—the basic features of modernity in these nine, historically staggered developments: (1) the privatization of re-

Totalitarianism, by contrast, whether Hitler's *Gleichschaltung* or Lenin's "democratic centralism," always involves *an attempt at the coercive politicization of diverse arteries of social interaction* such as the unions, the press, the police, sport, science, law, art, family life, education and, of course, the economy (Linz, 1975). This is what Franz Neumann referred to as "the destruction of the line between state and society" (1957, p. 245). And, in spite of much talk about "atomization," the capacious maw of the total state wants to swallow "subsystems," not granular individuals. Thus, the murderous brutality which accompanies fascist and communist regimes probably results—at least in part—from the factual discrepancy between a pluralistic infrastructure and a tightly integrated or unified political ideal. Since there was no such discrepancy in Greece (and here lies my objection to Popper), there could not have been any Greek totalitarianism, just as there could have been no Greek liberalism. The "line between state and society" did not yet exist, and thus could neither have been defended nor destroyed. This structural claim, it seems to me, provides an excellent starting point for a fresh understanding of Benjamin Constant's neglected thesis (n. 1) that transplanting the principles of ancient liberty into modern society inevitably produces tyranny.

Hannah Arendt and Leo Strauss seem to have had but little sympathy for each other's approach to Greek political theory. As suggested, however, the surface discord here conceals an underlying agreement, or at least a shared obsession with the "quarrel" between the ancients and the moderns. Both Arendt and Strauss, perhaps under the influence of Martin Heidegger, interpreted the self-destruction of the ancient world and the eventual transition to modern society as signaling a lamentable "loss" of depth and fullness. While Heidegger often speaks in this context of a "forgetfulness of Being," Arendt tends to view modernity as tragically marred by a forgetfulness of Aristotelian *praxis*, while Strauss views it as the sad product of amnesia about Platonic *noēsis* and especially about the intellectual grasp of "natural law" and humanity's highest Good.

The bizarre implication of all this is that modern society is based on a "mistake" or error of attitude, for example, on the hubristic "misinterpretation" of nature as a field to be exploited rather than a fount to be adored. One amusing claim found in Strauss' work is that the ancients entertained modernization (including Galilean science) as a hypothetical future on which to embark, but rejected it because they knew it would make them unhappy (1968a, p. 190). What concerns me here is not so much this particular claim, but rather the assumption implicit in it and in fact underlying much of Strauss' thought: that the fundamental

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ligion, (2) the rise of territorial nation-states with increasingly bureaucratic administrations, (3) the emergence of rational capitalism, (4) the specialization of science on the basis of rigorous quantitatively-hypothetical-experimental techniques, (5) the release of art from civic and religious functions, (6) the democratization of mass politics through representative institutions and the eventual stabilization of a universal franchise, (7) the shrinkage of the basic kinship unit to the small and increasingly one-generational nuclear family, (8) the birth of universal-compulsory education, and (9) the positivization of law or the shift in the basis of legality from immutable "natural law" to formal procedures for changing legal codes in an orderly way.

[8] It is Hannah Arendt (1968, pp. 143–71), and not Popper, who lumps Epictetus together with Mill. Of course, there is some basis for this misreading in Mill's text, since he does echo the Protestant self-deception that reformed religion is purely inward and devoid of a worldly, organizational and communicative dimension.

difference between antiquity and modernity can be traced to a *shift in attitude* or in the general climate of opinion. In other words, Strauss deliberately slights those *structural and institutional transformations* which make it rather pointless to place an ancient "regime" next to a modern one and solemnly ask, which is better?[9] Because ancient and modern systems of government present solutions to very different problems, of course, such direct "evaluative" comparisons are notably unilluminating. Strauss, no doubt, would reject my premise here, asserting that the problems confronting the ancients and the moderns are *indeed* different but not *so* different as I suggest, human nature having remained essentially the same. Be that as it may, my less controversial claim is only this: by associating, for example, Nazism with modern scientific detachment,[10] Strauss has helped promulgate the myth that twentieth-century autocracy is *basically* a result of our having stopped brooding over old books and having therefore forgotten the ancestral pieties. At the very least, it seems clear, this line of thought has deflected theoretical attention away from the possibility of an alternative hypothesis: that modern "totalitarian" regimes rely for ideological legitimation on a diffuse rancor against modernity and on an anachronistic nostalgia for the integrated and heavily politicized life of the Greek city-state.

Something similar can be said about Hannah Arendt. Unlike Strauss, of course, Arendt places her analyses of Greek politics and modern totalitarianism within the context of at least a rudimentary theory of social evolution. Modernization, she claims, can be summed up as "the rise of the social," the disappearance of political life and the atomization or privatization of the public sphere (1958a, pp. 38–49). Isolation, "massification" and the loss of civic spirit are the immediate preconditions for the emergence of totalitarian "movement-regimes," or so she claims (1958b, pp. 305–26). Now there is no question of doing full justice here to her intricate analysis. My concern is simply to point out that by linking totalitarianism so closely with "the loss of the political," Arendt too has been forced to neglect the plausibility of Benjamin Constant's thesis that the most appalling form of modern tyranny may actually contain a distorted echo of ancient freedom. She could have corrected this blind spot, I believe, if she had replaced her simplistic theory of the "rise of the social" with a theory more adequate to the increased structural differentiation of modern society. This possibility, in any case, is the one I will try to sketch out below.

As may be clear from these all too brief remarks, my sympathies are divided among Popper, Arendt and Strauss. Against Popper, I agree with the latter two that Plato and Aristotle were not totalitarians, although I justify my agreement here by the argument that liberalism is a specifically modern development, parasitic on the highly articulated and centrifugal character of the modern institutional order—and that there were not and could not have been any "Greek liberals" (though there were "democrats" in a variety of senses) for Plato to attack, just as there was no highly differentiated society which he might have wanted to "unify" by mass mobilization and terror. On the other hand, I would like to emphasize with Popper, and without much regard for the piety toward Greece informing the work of Arendt and Strauss, that there is a totalitarian potential inherent in the basic principles of Aristotle's and Plato's political philosophies—a potential only fully unleashed in the industrial-bureaucratic nation-state, but which was already perceptible as a threat in early post-Reformation Europe. This, I believe, explains liberal "forgetfulness" toward Greece (the kind we find in Locke, Montesquieu, Adam Smith, and James Madison—and which thus has nothing to do with ignorance of the past), and even encourages more. In this case too, however, I am far from being in complete accord with Popper, for I discern totalitarianism not in the texts of the

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[9] Although he never seems concerned with the despotic potential inherent in classical Greek political theory, Strauss does at times echo Benjamin Constant. But even when he does so he reveals the extent to which his interest in transformations of attitude overshadows his concern for structural change: "the relative success of modern political *philosophy* has brought into being a kind of society wholly unknown to the classics, a kind of society to which the classical principles as stated and elaborated by the classics are not immediately applicable" (Strauss, 1977, p. 11, my emphasis).

[10] This is what Strauss has to say about the logical disjunction Weber discerned between descriptive social theory and political partisanship, between stating that different decisions have different costs and actually making one decision and paying its particular costs: "Weber's thesis necessarily leads to nihilism or to the view that every preference, however evil, base or insane, has to be judged before the tribunal of reason to be as legitimate as every other preference." And he goes on to add: "In following this movement toward its end we shall inevitably reach a point beyond which the scene is darkened by the shadow of Hitler" (Strauss, 1953, p. 42).

Republic, the *Laws* and the *Politics*, but only in the imprudent importation of doctrines there espoused into the context of a highly differentiated modern society.

The general methodological premise underlying my argument thus far might be reformulated as follows: the normative claims of political philosophy, so I believe, can never be understood from behind a self-imposed veil of ignorance, but rather must always be interpreted in light of historical information about the institutional order within which these claims are to be enforced. My more specific assumption is that an account of the increasing structural differentiation of modern society provides an illuminating framework for discussing the old and vexing question of the relation between Greek political theory and modern totalitarian autocracy. We are now in a position to examine this claim in greater detail.

III

As I have said, the basic moral doctrines unfolded in the political writings of Plato and Aristotle seem inseparably linked to a series of "side premises" about the organizational structure of the city-state. I now want to examine what I take to be the most important of these auxiliary hypotheses. It is expressed most clearly in Aristotle's claim that "the polis is a compound like any other whole made out of parts, and these are the citizens who compose it."[11] The assumption is that society can be adequately grasped through the conceptual pair of "whole" and "parts."

It seems a pardonable oversimplification to locate the basic principle of Greek politics in the idea that totality is prior to individuality or that the *polis* is prior to the *politēs*. Such an idea, moreover, was no monopoly of pro-oligarchic philosophers like Plato and Aristotle, though it certainly received its historically most notable formulation through them. Even in democratic cities like Athens, the institution of ostracism revealed to what extent citizens had no "subjective rights" against the city.[12] This idea was formulated in a plethora of ways. Aristotle claimed, for example, that human beings are fundamentally political animals and that they can fully realize themselves only in political participation. He also said that politics is man's essence, that political science (*hē politikē*) studies everything of human value and that all spheres of life should be subordinated to political concerns. Even that "proto-liberal" Pericles seems to have had this idea: "We do not say that a man who takes no interest in politics is a man who minds his own business. We say that he has no business here at all."[13] These ideas all culminate in an emphatic subordination of individual rights to civic duties. And after all, how can individuals fear being impinged upon by their own essence? Why would they seek a "right" to lose their community?

In recalling the two principles that the *polis* is prior to the *politēs* and that rights are subordinate to duties, I have yet to say anything specific about social structure. Nevertheless, both claims (at least as Plato and Aristotle make them) seem to depend on the assumption that society can be accurately described as a "whole" made out of "parts." Indeed, the city-state appears to have been commonly conceived as an encompassing totality or ensemble of which the city dwellers were thought to be the constituent elements or living parts. Only in the light of this "empirical" premise, in any case, is it possible to understand the kind of "participation" glorified by Plato and Aristotle.

Plato, it cannot be denied, was an enthusiast of political participation.[14] But what did this imply? In Plato's "fully just *polis*," each individual plays his or her part, attends exclusively to a rigidly prescribed task and stays unobtrusively in place. Participation, as Plato admired it, was not collaboration in group decision making, but rather *to ta hautou prattein*,[15] simply to do that which belongs to one's *part*. Furthermore, if we abstract for a moment from Plato's appeal to the timeless

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[11] *Politics*, 1274b 39–40.

[12] As Moses Finley formulates this point: "Classical Greeks and Republican Romans possessed a considerable measure of freedom, in speech, in political debate, in their business activities and even in religion. However, they lacked, and would have been appalled by, inalienable rights. There were no theoretical limits to the power of the state, no activity, no sphere of human behaviour, in which the state could not legitimately intervene provided the decision was properly taken for any reason that was held to be valid by a legitimate authority" (Finley, 1973a, pp. 154–55).

[13] Thucydides, *The Peloponnesian War*, Bk. II, l. 40.

[14] This claim must be modified, of course, by consideration of the solitary, almost entranced, quality of Platonic *noēsis* and the small-scale and apolitical character of Socratic *elenchos*.

[15] *Republic*, 433b and passim.

Forms (which I cannot consider here) we will see that his most persistent complaint against "injustice" is that it is dysfunctional. He blames it for splintering *homonoia* (being "of one mind," "in agreement with oneself"), both civic and personal, necessary for coordinated action. And *homonoia,* like its close kin Platonic justice, requires everyone to do an assigned chore.

For Aristotle too, political participation demanded that an individual's existence be ingredient in the *polis,* or, in Constant's words, *englobée dans l'existence politique* (1861, p. 557). The statement that "the *polis* is prior to the individual"[16] stands at the opening of the *Politics,* and that book is full of arguments to the effect that "we must not suppose any one of the citizens to belong to himself, for they all belong to the *polis* and the care of each is inseparable from the care of the whole."[17] Participation in the mixed constitution or *politeia* favored by Aristotle has a great deal more to do with collective deliberation and policy making than does the chore-minding of Plato's ideal city. Nevertheless, both regimes are conceived to be all-encompassing. Both, for example, are compared to bodies from which one cannot (successfully) detach a foot or hand.[18] For the "part" played by the morally enmeshed individual, both Plato and Aristotle use *to morion,* a word closely related to *moros* and *moira,* one's appointed and fore-ordained destiny.[19] The background implication, about which much could be said, is that political portions are somehow allotted to individuals on the basis of incorrigible nature. And the unchallengeable messages sent by nature (for example, information that someone has a cobbler's or a guardian's soul, is a natural ruler or slave) are ultimately concocted to reinforce the whole/part interpretation of society. This is the case, so Plato (for example) believes, since the kind of soul I indelibly have can only unfold its potential in the context of a beehive division of labor.[20] The "physical" ascription of roles lets *polis*-dwellers know that they had better participate, had better stay piously inside the *polis* just as any other part must selflessly adhere to its embracing whole.

Now, one of the ideas in the classic texts of Greek philosophy which at first seems quite similar to "representation" turns out to be as unlike its modern counterpart as *to ta hautou prattein* is unlike participation at the polls. It too depends on the whole/part schematization of society, and especially on the direct corollary that "in all things which form a composite whole and which are made up of parts . . . a distinction between the ruling and subject element comes to light."[21] Conjoin this premise to the further claim that "a *polis* or any other systematic whole is most properly identified with the most authoritative element in it"[22] and you have a Greek idea of "representation" which both Plato and Aristotle seem to have thought a powerful device for legitimating the hierarchical order of society. They both tried to make rigid stratification acceptable, of course, by comparing the city to the soul, and then reminding us that "reason" must surely rule over the passions and appetites because it is "higher" than they are. The basic political idea, in any case, was that the worthiest members of the polity, the *maiores partes,* "represent" the whole in the sense that they embody the essential qualities of the *polis*-dweller to the highest degree. The plausibility of Aristotle's *pars pro toto* identification of the *politai* or full-fledged citizens with *polis*-dwellers in general depends directly on this "hierarchical" interpretation of the city, whereby the topmost elements of the unified status-pyramid "sum up" the ordered ladder below.

Even when we ignore the shadowy relation between citizens and noncitizen metics or slaves, and concentrate instead on authority relations within the citizen-body, Aristotle seems to justify "representativeness" in a similar fashion. Behind his preference for the constitution called *politeia* (the best regime possible under actual conditions) lies the idea that middle-class citizens *should* dominate the city since they can avoid the ingrained hubris and contempt of the wealthy as well as the ground-in servility and envy of the poor. Thus knowing how to rule and be ruled in turn, they *represent* the "best" in man, the ability to live and act according to "the mean."

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[16] *Politics,* 1253a 25–26.

[17] *Politics,* 1337a 27.

[18] *Republic,* 462 c–d; *Politics,* 1253a 19–23.

[19] For more detailed etymological information, see Cornford (1957). Since the idea of a *physical* ascription of roles is patently "nonliberal," it was a serious blunder for Donald J. Allen to make it the basis for his defense of Aristotle's underlying "liberalism" (Allen, 1965).

[20] It is crucial to distinguish the generic concept "functional differentiation" from its sub-class "division of labor" which is indispensable for analyzing even relatively undifferentiated societies. Modern social differentiation, as Georg Simmel explains so well (1955, pp. 127–95), involves the multiple participation of each individual in incongruous and cross-cutting social circles. The distinction between general social differentiation and the differentiation (within the economy) of specific professional groups seems to have been fudged by Durkheim in his otherwise powerful analysis of social differentiation (1933).

[21] *Politics,* 1254a 29–31.

[22] *Nicomachean Ethics,* 1168b 31–32.

IV

As should now be clear, the claims of Aristotle's *Politics* which interest me most are the notorious ones: that the *polis* is prior to the individual, that human beings are born for citizenship in a city-state, that ethics and politics coincide or that "political science" studies everything of human value. As has frequently been noted, these claims all sound wildly implausible to modern ears, and so the first thing to do is to try to explain exactly what made them seem convincing to Aristotle. The *Politics* begins, as everyone knows, with an attack on Plato, specifically on the claim put forward in the *Statesman*, that there is little or no difference (so far as the exercise of authority is concerned) between a large household and a small *polis.*[23] From Aristotle's point of view, this was an extremely dangerous thought. Indeed the whole argument of the *Politics*, the very idea that the *polis* is prior to the individual and that it is the flowering and culmination of nature, depends upon a sharp contrast between actions which take place in an *oikos* or household and actions which take place in the political sphere. In the *Nicomachean Ethics*, one should recall, Aristotle says that practical reason depends upon a distinction between actions which are good for something else (useful actions) and actions which are good for their own sake.[24] Without insight into actions which are simply choiceworthy for their own sake, *phronēsis* or prudence would tumble into an abysmal infinite regress. The basic form which this distinction takes in the *Politics*, in any case, is the split or dualism between household or "economic" actions and public or political actions, or, in Aristotle's terms, between *technē* and *poiēsis* on the one hand and *praxis* on the other. Hannah Arendt has articulated this distinction neatly by contrasting the necessity and constraint of household production with the freedom of political deliberation and action (1958a, pp. 28–37). Household production is "technical" in the sense that it has its end or goal (biological survival or mere life) outside itself. Slaves struggle in a repetitive way to satisfy the daily demands of biological nature. Citizens (and Aristotle believes that only those household heads who own slaves should have the franchise) are able to *presuppose* the satisfaction of biological needs. As a result, they can leave behind the realm of necessity or "mere life" and enter into the realm of freedom or "the good life," the domain of citizen-sharing in collective deliberation through language, the domain of happiness, free choice and noble deeds. The negative side of Greek "freedom," in other words, is release from the repetitive constraints of biological life; the positive side is reciprocity, the chance for peers to rule and be ruled in turn. It is this emphasis on reciprocity or the "partnership in the good life"[25] which explains what Aristotle meant by the "priority" of the *polis* over the individual.[26] If a man lived exclusively in the Peiraeus, surrounded by the whirr of instrumental routines, if he were constantly occupied with activities relating to "mere life," he would never develop those statesmanlike characteristics which Aristotle associated with true humanity. He would not learn, for example, how to rule and be ruled in turn. When Aristotle claims that despots are worse off than citizens who share power (because there is nothing noble in giving orders about necessary things),[27] he is contrasting the unfulfilling nature of instrumental-manipulative action with the "end-in-itself" of communicative partnership and sharing.

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[23] *Politics*, 1252a 7–20; *Statesman*, 259b.

[24] *Nicomachean Ethics*, 1094a 18–22.

[25] *Politics*, 1325a 7. For the intimate character of this partnership, consider Aristotle's claim that "if the citizens of a *polis* are to judge and distribute offices according to merit, then they must know each other's characters" (*Politics*, 1326b 15–16).

[26] The rise of Macedonia (which Aristotle lived to witness) marked the beginning of the end of the Greek *polis* as a significant unit of political and military life. It is in this context, and with some reference to Socrates' daemon, that I want to view Aristotle's evaluation of the life of *theōria* or contemplation as "the complete happiness of man" (*Nicomachean Ethics*, 1177b 24). Although he explicitly says in Book 1 of the *Nicomachean Ethics*, that *autarkeia* (defined as "that which, when isolated, makes life desirable and lacking in nothing" [1097b 15]) is unrelated to the *bios monōtos* or solitary life—the reason being that "man is a political animal" (1097b 9–11)—in Book 10, he contradicts this statement by saying that the wise man or *sophos* has more *autarkeia* than others simply because "he is able to theorize all alone" (1177a 32–33). Although this is a contradiction of sorts, it may be explained somewhat by Aristotle's need to find a form of happiness which might survive the imminent collapse of the *polis.* Similarly, the Christian tradition of apolitical inwardness was always able to perceive itself foreshadowed in the ideals of contemplative withdrawal and ataraxia current in Hellenistic times. In one sense, at any rate, Aristotle never swerves from his definition of man as a political animal. He specifically calls *theōria* a divine and not a human *aretē* (1177b 32). For a carefully documented account of Aristotle's less than total commitment to the theoretical life (whose devotees "do not seek those goods which are specific to man" [1141b 8]), see Cooper (1975).

An additional factor explaining the "naturalness" attributed to the *polis* is the unique mortality reserved for human beings in colloquial Greek. It we want to explain in Aristotelian terms why only human beings were called *hoi brotoi* or "the mortals," we should recall that for Aristotle all things were made out of a conjunction of form and matter. When an animal died, its form and matter were wrenched apart, but both elements "survived," the matter in a decaying corpse, the form in the species (since there was no evolution). The form or universality of human beings, on the other hand, was thought to be strictly individual. It was the kind of thing which could only be captured in a life story. Its survival was guaranteed by no species, but could at best be achieved by an individual's acting in a memorable way. When Aristotle says that *kala erga* or *kalai praxeis* are the end and purpose of the *polis*,[28] he is suggesting that noble deeds are choiceworthy, in part because they allow men to overcome their mortality and to leave a trace in history—like the heroes of Marathon. According to this line of thought, what makes slavery unendurable is simply the fact that slaves have no chance to leave a trace in history.

Now although this Arendtian claim is quite striking, it should not be allowed to deflect attention from the simpler argument, found throughout Aristotle's practical philosophy, that friendship, partnership and sharing in the good life are worth choosing for their own sakes. It was his supreme concern for *reciprocity*, to be sure, which motivated Aristotle's insistence that a good city be small enough to be "easily surveyed."[29] To this he added the famous demand that all the citizens be able to look at one another, and even to know each other's characters intimately. The "unrealistic" quality of these demands has led numerous historians (following Popper) to dissociate totally the philosophical claims found in the classic texts of Greek political theory from the ordinary life of democratic city-states. Such a dissociation, however, while not entirely unjustified, should not be exaggerated.

V

The "priority" of the *polis* over the individual, in point of fact, was *not* invented by philosophers. Athens in the classical age, to be sure, did not correspond to the idealized image of a continuous open-air mass meeting. The one thousand square miles of Attica were not "easily surveyable," and its population of sometimes up to 40,000 adult male citizens far exceeded the limit placed on philosophically "ideal" states. Nevertheless, Athens was, like other *poleis*, "a small, homogeneous, relatively closed face-to-face society" (Finley, 1973, pp. 30–31) and its highly integrated character lent a degree of everyday plausibility to Aristotle's rather abstruse philosophical claims.

The colloquial word *politeia*, for example, meant simultaneously the constitution, citizenship, the citizen body, the daily life of the city, administration, a tenure of office, a course of policy and the general structure of the *polis* (Liddel and Scott, 1940, p. 1434). As Victor Ehrenberg summarizes this point:

> The use of the same word for individual participation in the state and for its general structure shows that participation was not in the main a purely legal act between individual and state; it reflected the vital adherence of the individual to the citizen body, as also to the other communities inside the state (1964, p. 39).

The *polis*, in other words, was a moral, not a legal, community.[30] Its institutions were commonly said to inculcate virtue and to make the citizens "good." This is why the part was unblinkingly subordinated to the whole, indeed why the whole/part schema seemed applicable to Greek society in the first place. Feeling free, at least after the Greek victory over the Persians, seemed tangibly dependent on political fraternity, solidarity and collaborative self-government (Pohlenz, 1966). It carried little connotation of an absence of collective interference in individual lives.[31] The normal democratic polity (and not just Plato's oligarchic dream) guaranteed and encouraged participation in the sense of "vital adherence" of the individual to the community. Democratic institutions such as the lot, short rotating terms of office, and the regular "scrutiny" of officials at end of term all encouraged first-hand participation. In Athens, at least, pay for attendance stimulated citizens to take active roles in the law courts, the council, the magistracies and the sovereign Assembly (Jones, pp. 5–6). By mobilizing public opinion, the city promoted competition by the rich in the financing of public liturgies, the manning and maintaining of war-triremes and so forth. In sum, it told the citizens how best to play their parts.[32] What formal legality there was, moreover, tended to be submerged in the overriding context of moral fellowship. The large popular juries in democratic policies like Athens did not always vote according to standards of codified and predictable legal justice. When a decision like the condemnation and summary execution of the generals in command at Arginusae violated formal procedure, it was justified by appeal to the security and prosperity of the city.[33] Even Aristotle, who explicitly condemns the Athenian tendency to override "law" with "popular decrees," says: "We call those things just which produce and preserve happiness for the political community."[34]

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[27] *Politics*, 1325a 25–27.

[28] "It is for the sake of *kalai praxeis*, beautiful and noble actions, and not for the sake of surviving together, that political associations exist" (*Politics*, 1281a 2–3). See also *Nichomachean Ethics*, 1161a 34–35.

[29] *Politics*, 1326b 25.

[30] It was a commonplace in nineteenth-century England that anyone curious about Athens only needed visit Oxford to discover what it had been like. Perhaps this is what Sir Ernest Barker meant by his not totally inaccurate claim that "the city was not only a unit of government, it was also a club" (1918, p. 16).

What I want to emphasize here is the fraternal or corporative quality, the *homonoia* which most Greeks (not just "illiberal" philosophers) discerned at the root of political life. *Societas* and *communitas* are the traditional Latin translations of the Greek *koinōnia* or "association." The noun *koinōnia,* in turn, is directly related to the adjective *koinos* meaning "shared in common" (Riedel, 1975, p. 31 ff.; Mulgan, 1977, pp. 13–17). And according to Aristotle again, what men share in common, holds them together. A *koinōnia,* in other words, was ordinarily understood as an organic association expressing a corporate bond.

Aristotle's whole/part schematization of society, in other words, was not merely abstruse and unrealistic "philosophy." It gained plausibility against the background of an everyday Greek identification of the polity with total society. In the city-state, most spheres of social life had political connotations or overtones. The Periclean building program alone reminds us of the extent to which Greek art was civic art. The conventional Olympian religion, at least, was a civic religion. Indeed, it was almost a self-deification of the *polis.* As a result, impiety was a crime against the state. Law was public and largely nonprofessional. The economy was likewise "submerged" in a surrounding nexus of political and military concerns. Weber referred to the *polis* as a "warrior's guild" (1958, p. 220), thinking of Sparta, no doubt—and it is especially true of such cities that even sport and gymnastics had a heavily political tinge. But there seems to have been nothing unusual about the fact that the Athenian Alcibiades' victory in a chariot race at the Olympic games was an integral part of his *political* career (Finley and Pleket, 1976, pp. 104–07).

The claim here is merely that the city-state, as an ideal type, was *relatively* undifferentiated. That it was not completely undifferentiated goes without saying. The fact that the Spartans had to encourage homosexuality, for example, reveals that the family was perceived as a distraction from and threat to the intimacy and coordination of the hoplite phalanx. And of course, the constant reaffirmations of "unity" found in Greek political writings testify to the divisive force of party factionalism and class strife in the ancient city. In any case, the vague politicization of most spheres of social life lent a degree of everyday plausibility to Aristotle's otherwise obscure philosophical claim that all (subordinate) communities are "parts" of the political community. It should now be easy to see what Aristotle meant by his claim that ethics and politics coincide, or that political science is the master or "architectonic" science and studies everything of human value. Everything important in the city-state seemed to go on within the political sphere. Or in Weber's terms: the "total status" (or essential humanity) of the citizen was contained *inside* the single system of the polity. As a result, political "participation" seems to have had the force of a direct moral imperative. It was not justified by any detour of Hobbesian calculation on the part of discrete individuals who longed for security and feared death. It was simply a life worth choosing in and for itself.[35]

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[31] In a book heavily influenced by Constant's understanding of the difference between ancient and modern liberty, we read the following: "The state allowed no man to be indifferent to its interests; the philosopher or studious man had no right to live apart. He was obliged to vote in the assembly, and be magistrate in his turn. At a time when discords were frequent, the Athenian law permitted no one to remain neutral; he must take sides with one or the other party. Against one who attempted to remain indifferent, and not side with either faction, and to appear calm, the law pronounced the punishment of exile with confiscation of property" (Fustel de Coulanges, 1956, p. 267).

[32] The precise kind of participation encouraged by the state, of course depended on the relative degree of democratization. Consider, for example, the following: "The Athenians also took part in the Assembly, which had 40 ordinary meetings a year, and public slaves rounded them up from the agora onto the Pnyx for these meetings with the aid of a ruddled rope: anyone found afterwards with red on his clothes was fined" (Webster, 1969, p. 69).

[33] Xenophon, *Hellenica,* Bk. I, Ch. vii.

[34] *Nicomachean Ethics,* 1129b 17–19. Later, after declaring that "friendship holds cities together" (1155a 22–23), Aristotle goes so far as to say that "among friends there is no need for justice" (1155a 26–27).

Of course, the common Greek practice of enslaving the entire population of conquered cities must have been a factor in the everyday identification of individual freedom with the autonomy of the city. The threat of foreign military power must have made palpable the dependence of all "parts" on the political "whole." This was the case even in Aristotle's "imperfect" polities. Aristotle, one recalls, classified democracy among the "perverse" constitutions, because it granted the franchise to unworthy men. As a consequence, he considered the best democracy to be a democracy of farmers, of men whose continuous labor far from the city would surely impede their daily participation in collective deliberation and policy making.[36] Yet, the possibility of enslavement would have been terrifying enough to transform even such hierarchical half-enfranchisement of "the many" into something resembling total inclusion.

Hegel once wrote in the margin of his own copy of the *Philosophy of Law* (1955, p. 417) that "the ancients knew nothing of conscience," perhaps thinking of the fact that when Socrates tried to introduce his private "voice," his daemon who had no place in the public cults, Athens expunged him from its midst. And indeed the modern distinction, so important to Kant, between morality and legality, between inward conviction and outward conformity seems to have had little intelligibility for the Greeks (Kant, 1965, p. 19). As Aristotle sees it, in any case, ethics is "out in the world," is firmly rooted in *ēthos*, in the customs, traditions and institutions of the political community. An education toward virtue is a process of habituation which takes place out in the public domain. You cannot become good by listening to one of Plato's lectures on "the Good," Aristotle warns.[37] You must be habituated into the worldly activities of ruling and being ruled in the political sphere. This line of thought, of course, provided yet another justification for viewing the *polis* as "prior" to the citizens, and thus for conceiving of the polity as an all-encompassing whole made out of citizen parts.

VI

In his *Memorabilia,* Xenophon relates an illuminating conversation about the priority of the *polis* over the individual which supposedly took place between Socrates and his "disciple," the North African voluptuary and sophist, Aristippus. Their exchange begins (as usual) with Socrates trying to convince his interlocutor that self-indulgence is logically absurd, but soon turns to a more interesting discussion of the idea that the only life worth living is the *bios koinōnos,* the collaborative life of citizen interaction within a *polis.* Aristippus, setting an example for the Cyrenaic school which will follow him, repudiates this collectivist ideal. The crippling responsibilities of ruling, he argues, make it a flimsy disguise for slavery:

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[35] Many ordinary Greeks, in other words, and not just "totalitarian" philosophers, must have reacted with astonishment to the contractarian attitude which Popper admires in Lycophron (Popper, 1966, Vol. I, p. 114 ff.). We can perhaps experience the same sort of astonishment by considering the apparent perversity of Kant's claim that marriage is essentially a relation of contract and exchange (1922, pp. 91–94). Hegel, Athenian at heart, was absolutely outraged at this suggestion (1952, p. 58). Institutions like the *polis* and the family, he argued, do not simply protect individual rights. Rather, they provide otherwise unattainable possibilities for self-realization in partnership. This explains how, as institutions, they can be said to have "moral" content. They are media for creative collaboration, not mechanisms for manipulative and possessive solitude.

[36] *Politics,* 1318b, 6–16.

[37] *Nicomachean Ethics* 1095a 1–7.

> For *poleis* claim to treat their rulers just as I claim to treat my servants. I expect my men to provide me with necessities in abundance, but not to touch any of them; and *poleis* hold it to be the business of the ruler to supply them with all manner of good things and to abstain from all of them himself.[38]

The ignoble constraints of being ruled, he goes on to say, are even more unbearable. Political participation, viewed with a cool head, is always self-incarceration. Freedom, he concludes, is incompatible with both ruling and being ruled. It requires us to take a "middle path," the path of the metic, the alien or rootless freeman whose happiness depends on his having no citizenship and no assigned place. "I do not shut myself up in the four corners of a *politeia,*" Aristippus gravely announces, "but am a stranger in every land."[39]

To this outburst and before returning to his conventional homilies about virtue being more choiceworthy than vice, Socrates pointedly answers: yes, political life is difficult; but a life without friendship (*philia* in the special Greek sense of a "public virtue" akin to solidarity), without partners or allies is more wretched still. Watch out Aristippus! The highways are dangerous. *Vae soli!* Woe to he who goes alone![40]

I take Aristippus as emblematic for my argument in this article for a cluster of reasons, all related to the fact that he is a "part" who (viciously enough) seems to feel no need to be incorporated into the "whole." He is Aristotle's *apolis,* the cityless man said to be either a beast or a god.[41] Significantly, his aloofness from politics does not seem utterly nonsensical to modern readers, even though it was obviously meant to appear outrageous and perverse in its original Athenian context. Indeed, as depicted by Xenophon, Aristippus is exceptionally well-suited for summarizing our argument thus far and for focusing attention on what is at stake in the claim that there is a totalitarian potential inherent in Greek political thought. Because of the evasiveness and self-seeking smallness of his apolitical turn, for example, Aristippus reveals the weakness of Popper's notion that apolitical arguments in ancient Greece were fundamentally liberal, "humanitarian" and pro-democratic. Aristippus also helps us understand that Arendt is right in viewing Aristotle's repudiation of cityless life as merely the obverse side of his affirmation of freedom, happiness and the partnership in the good life. The Aristotelian *polis* was unquestionably meant to widen the scope and deepen the significance of human life beyond the mechanical satisfaction of biological needs. On the other hand, Xenophon's Aristippus helps us keep clearly in mind the colloquial and pervasive Greek identification of polity with society. *Apolitical* action, even in Periclean Athens, bore the vague stigma (or halo) of being *asocial.* This of course, is no longer the case today. In spite of Arendt's views about the modern privatization of everything, the centrifugal and highly differentiated *communicative* network within which we live and act cannot be adequately depicted as a loose assemblage of dissociated atoms. Just so, our multiple channels of social communication make Strauss' "basic question" ("politics or philosophy?") seem similarly obsolete. We have such a plurality of choices confronting us that the simplistic alternative political/apolotical (whether "apolitical" is taken in its hedonistic-epicurean or in its Socratic-contemplative version) is no longer so overwhelmingly crucial as Strauss seems to have believed. In sum: Aristippus helps us understand that modern liberal sympathy for apolitical ideals is based not so much on philistinism and superficiality of attitude as on a specific set of historical transformations—*structural* changes which eventually made it unrealistic to conceive of Western societies as civic wholes made out of citizen parts.

VII

The first institutional development to undermine radically the classical whole/part schematization of society was the split between *imperium* and *sacerdotium* which lies at the heart of medieval Christendom. Even the conversion of Constantine and the declaration of Christianity as a state religion did not reverse this differentiation, since Christianity (as Machiavelli lamented) was not well suited for the role of a solidarity-ensuring *theologia civilis.* Behind the *imperium/sacerdotium* dualism, no doubt, we can discern the double heritage of Israel and Greece. Judaic culture, one might say, was marked by a "totalization" of religion parallel to the "totalization" of politics witnessed in Greece. It might not be simplifying too much to say that one of the most striking achievements of Christianity was its successful integration of these two seemingly "contradictory" strands of ancient civilization. This amalgam in turn produced what could well be described as one of the "secrets" of political rationality in the West: the historical unlikelihood of a double totalization.[42] Throughout the Middle Ages people were confronted with two distinct and quite incommensurable realities: the towering stone castle and the towering stone cathedral; Caesar and Christ; sovereignty on the one hand and salvation on the other. The claim to a kind of universality by both politics and religion led, by the end of the fifth century, to the Gelasian doctrine of the "two swords." This doctrine eventually became entrenched as the explicit Western alternative to Byzantine caesaropapism or the Eastern tendency to transform the church into a civil institution. The development of anchorite monasticism might even be understood as a strategy for circumventing the danger, which did arise, of the Church losing its independence and becoming a pliant tool of secular authorities (Parsons, 1971, p. 33). It eventually became common in the West to regard any unification of sacred and secular authority as a backsliding into paganism and perhaps as a ruse of the devil.

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[38] Xenophon, *Memorabilia,* Bk. 2, Ch. 1, l. 9. Aristippus was notorious in antiquity for having been the first of Socrates' disciples to accept payment for his teaching services—an innovation which was commonly thought to signal a betrayal of *both* otherworldly philosophy *and* collaborative politics. See "Aristippus" in Diogenes Laertius, Bk. 2, ll. 65–104.

[39] *Memorabilia,* Bk. 2, Ch. 1, l. 13.

[40] I will comment below (though only indirectly) on the curious similarity between Aristippus and Socrates justly stressed by Strauss (1972, pp. 32–39).

[41] *Politics,* 1253a 2.

Such incongruity, as the investiture controversy, the Inquisition, and the revocation of the Edict of Nantes all show, was no comfortable or foolproof solution. Yet it was one of the first and most crucial steps toward the clear *differentiation* between the polity and total society, toward the proliferation of quasi-autonomous social domains which eventually made it quite implausible to regard an advanced European society as a political whole "containing" individual human parts. Because individuals participated in both realms, in both faith and citizenship, they could not be incapsulated elements of a single totality. This is why Augustine views the human being as a "social" but not as a "political" animal.[43] In the *City of God,* of course, politics was assigned a strictly limited role *within* the general economy of individual salvation. But this too was a one-sided view.

In sum, because of the "temporal disjunction" between infinity and finitude, there was no longer any noncontroversial *whole* of which individuals (as psycho-organic units) could have been considered the constituent parts. It is this development, perhaps even more than increase in scale, which eventually disqualified modern society from being *eusunoptos* ("easily surveyable") as Aristotle says all good societies must be. Modern ideals of individual freedom and spiritual autonomy seem closely related to this major shift in the infrastructure of society. Located at the intersection of two "autonomous" domains, the individual became (to adapt a phrase of Simmel) a *tertius gaudens.*[44] Thus, the "other-wordly" church could protect an individual from encroachments by the state, while secular authority could defend him or her against the growing power of the ecclesiastics.

With the "bourgeois" differentiation between economy and polity—which started to gain real momentum in the eighteenth century, but had already sparked the beginnings of modern liberalism in the work of John Locke and others—this development was powerfully reinforced.[45] It became even more obvious that the polity could no longer be identified with total society, nor could society itself be conceptualized as an organic entity "incorporating" its various organs.[46] Plato's and Aristotle's emphasis on duties instead of rights, so I have been arguing, depended on the relatively undifferentiated character of the Greek social order. Our highly differentiated society, by contrast, makes Aristippus seem less perverse than Xenophon intended.[47] The ancient question "how can we live best?" is no longer susceptible to a purely political answer. Unlike Aristotle's *hē politikē*, our political science makes no pretence of studying everything of human value. No one considers "government" (our pale replica of *politeia*) to be coextensive with human life. Reasons of scale alone—as well as structural differentiation—make it absurd to hope that modern society could be constructed or "steered" like a conversation. Since politics is now often seen as primarily a conjunction of electioneering, lobbying and bureaucracy (i.e., large-scale housekeeping), it cannot also be conceived as a whole, much less as a society-encompassing whole, which could give moral security and "warmth" to its constituent parts.

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[42] With St. Paul's decision that entering the Jewish community and following Jewish law was no longer necessary for a Gentile to become a Christian, Christianity managed to break its ethnic, territorial and political moorings. It became a *civitas divina.* Thus, it could coexist with temporal powers (you could be a Christian in both Rome and Athens) while resisting integration into political states (Parsons, 1971, pp. 30–34).

[43] *The City of God,* Bk. 19, Ch. V. It is probably worth recalling that Thomas Aquinas too transformed Aristotle's *zōon politikon* into an *animal sociale. Summa Theologica* Pt. 1, Qu. 96, Art. 4.

[44] The "liberal" branch of corporativist theory (and I am thinking, for example, of Maitland and Figgis) focused on the *group* in its status as an intermediary between the individual and the nation-state. Although their main concern was to obviate the tyrannical tendencies of the hypertrophic state, this "medievalism," like the attendant talk of "small religious communities," has never seemed quite convincing. The idea of multiple corporations, at any rate, must be carefully distinguished from the notion of "structural differentiation" developed by Simmel to characterize the basic pattern of modern Western society. (Besides Simmel, 1955, see Simmel, 1971.) The point to make against the wistful corporatists is that, in modern societies, the individual cannot be "redeemed" in *any* group, in state, church, guild or whatever. The "bourgeois rights of man" can be understood as the constitutional correlate of structural developments which rendered obsolete the whole/part schematization of society by reinforcing human individuality at the intersection of several overlapping contexts of social communication. None of these contexts completely encircles or contains the individuals who act within it, and thus none can be considered an "organic corporation." In such a highly differentiated society, individuality begins to seem like an "autogenic" achievement of single persons who, without any socially approved pattern to follow, manage to integrate their diverse roles into coherent life stories.

VIII

Modern citizens, as Giambattista Vico had already noted in the early eighteenth century, "have become aliens in their own nations" (1968, pp. 378–79). To a society of civic *déracinés*, as I said above, Aristippus' apolitical ideal does not seem entirely unconvincing.[48] Few of us would be shocked or disappointed to hear that we are no longer parts of a political whole but rather, say, occupants of the political system's environment, an environment flatteringly called "the public."[49] In truth, the participation thought necessary for producing legitimacy in pluralistic-constitutional democracies usually boils down to suffrage. The act of voting, however, is just an occasional or periodic choice between competing administrative functionaries and policy makers by a "public" whose life is chiefly and focally *outside* politics, located in other social contexts such as science, religion, economy, family, education, art, law, and so forth. The rational core of modern liberal contractarianism, as a consequence, did not lie in the unbelieveable idea that some pre-social Crusoe could (before learning a language and so forth) be articulate about his needs and rationally "choose" to enter society. It lay rather in the historically fitting conviction that an individual who has a complicated life in various nonpolitical social realms can reasonably evaluate different sorts of polity.[50]

In modern society there is no longer any question of morally and humanly fulfilling participation for the mass of citizens inside the political system. As a matter of fact, the right not to participate in politics (part of what "negative freedom" means) is often viewed as a fundamental condition for our being able to live with some degree of happiness and dignity. At least this is what lends credibility to Nietzsche's claim, so shockingly unlike Aristotle's, that politics is "a field of work for the punier heads" (1966, p. 1133). The un-Greekness of such influential and compelling reinterpretations of political participation seems unmistakable. No one can "realize" him- or herself through mass ballot voting. As a result, the ancient ideal of a "full-time voter" sounds either silly or sinister to modern ears (Parsons, 1971, p. 105). Such a recognition, needless to say, does not require us to celebrate "voter apathy" as a sure sign of health in a democracy. The consequences of varying degrees of popular participation in political decision making cannot be safely evaluated in general and in advance. What the foregoing analysis can help us do, however, is to distinguish clearly, as Benjamin Constant did a century and a half ago (1861, pp. 558–60), between the specific and pre-channeled *influence* we want to exert on governmental policy and the much more exorbitant Graeco-totalitarian demand that citizens achieve their total or essential status within the political sphere.

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[45] The separation between property ownership and military, political and ecclesiastical functions only began in the sixteenth century with the "reception" of Roman law (Schlatter, 1951). One of the points to make here is that the importance of the economy in modern society depends directly on social differentiation, since capitalism could only "take off" after it had shed its anchorage in the surrounding social and religious nexus (Polanyi, 1957).

[46] The modern political arena, in other words, had dwindled to the lowly rank of a subsystem among subsystems. It has become, in Nietzsche's words "the so-little-as-possible state" (1966, p. 1133). Because of this development, human engagement can only be partial and incomplete. This point about politics is just another example of Max Weber's more general claim that, in modern society, the "total status" of the individual is never contained in any single interaction context. For this Simmelian argument, see Weber (1954, pp. 105 ff.).

[47] Even in antiquity, of course, and especially after the battle of Chaeronea in 338, there were those who went along with Aristippus' glorification of the *apolis*. Their rejection of *koinōnia*, however, was often tinged with resentment and understandable as a foreshadowing of Christian inwardness.

[48] Indicative of modern civic "uprootedness" is the contrast between Aristotle's and Locke's reasons for condemning suicide. For Aristotle, suicide is wrong because it betrays the community; for Locke it is wrong because an individual's survival is "God's property" and hence not to be tampered with (*Nicomachean Ethics*, 1138a 5–14; *Second Treatise of Government*, Ch. 2, Sec. 6). Of course, the classical study of suicide and the difficulty of dealing with it on the basis of the Protestant "ontology of privacy" is Durkheim (1951).

[49] For one of his many attempts to replace the old whole/part schema with a system/environment model for the study of society, see Luhmann (1965).

[50] Vestigal Aristotelianism may have contributed to a peculiar lack of clarity in early formulations of contract theory. Even that arch anti-Aristotelian Hobbes suggests that the dissolution of government cannot be distinguished from the dissolution of society, thereby in one sense perpertuating the ancient identification of the polity with total society.

IX

In summary and conclusion: the basic principles of Greek political theory were inextricably coupled to a series of auxiliary claims, most notably to the empirical premise that society can be adequately grasped as a "whole" made out of "parts." This premise, in turn, was rendered plausible by the *relatively undifferentiated* character of the Greek city-state, where art was largely civic art, religion was civic religion and so forth. With the increase in social differentiation accompanying first the rise of Christianity and eventually the emergence of "bourgeois" capitalism, the whole/part schematization of society became definitively anachronistic. As a direct result, the basic moral claims found in the political writings of Plato and Aristotle (such as the emphasis on duties instead of rights and the subordination of the individual to a single overarching community) lost their initial core of rationality. Thus, liberal "forgetfulness" (not ignorance!) of Greece—or so I want to claim against Arendt and Strauss—is partly justified by the immense increase in structural differentiation characterizing modern society. The same transformation—and here I part company with Popper as well—helps explain how the originally convincing principles of the moral *polis* came to be enlisted in ideological support of what Mill aptly called the moral police.

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On the Proper Use of Ancient Political Philosophy: A Comment on Stephen Taylor Holmes's "Aristippus in and out of Athens"

JAMES H. NICHOLS, JR.
New School for Social Research

I

The central concern of Stephen Holmes' article is neither with theories that seek to describe contemporary society, nor with the teachings of ancient Greek political philosophy, but with the relation between Greek political philosophy and contemporary society. Two strands of thought run through the article's argument. On the one hand, modern society has become so obviously different in so many ways from the Greek *polis* that the conceptions of Greek thought should be discarded as timeworn and obsolete. In accordance with this view, Leo Strauss' arguments for the serious reconsideration of the ancient philosophic understanding are lightly dismissed as "amusing claims," "rather pointless" questions (however "solemnly" asked), and mere calls to remember "ancestral pieties" (Holmes, 1978, pp. 116–17). On the other hand, the appeal of Greek political philosophy today is dangerously powerful and tends to produce the evil consequences of totalitarianism. So great is the allure of Greek political thought, says Holmes, that Hannah Arendt, author of the most profound book on the origins of totalitarianism, failed to see how her "piety toward Greece" might increase the danger of what she cared most deeply to avoid (p. 117).

How, one wonders, can obviously obsolete thought have so powerful an allure for serious thinkers? One answer to this question might be that the obsoleteness of Greek conceptions is not obvious, but needs to be revealed by Holmes' analysis. This answer, however, cannot suffice, for virtually every literate political scientist is aware of the massive differences between the Greek *polis* and our contemporary societies, which produce the obsoleteness of Greek thought as Holmes understands it. Even Leo Strauss, who is alleged to be singularly neglectful of social structure, states with complete clarity the immediate inapplicability of Greek principles:

> We cannot reasonably expect that a fresh understanding of classical political philosophy will supply us with recipes for today's use. For the relative success of modern political philosophy has brought into being a kind of society wholly unknown to the classics, a kind of society to which the classical principles as stated and elaborated by the classics are not immediately applicable. Only we living today can possibly find a solution to the problems of today. But an adequate understanding of the principles as elaborated by the classics may be the indispensable starting point for an adequate analysis, to be achieved by us, of present-day society in its peculiar character, and for the wise application, to be achieved by us, of these principles to our tasks (Strauss, 1964, p. 11).

Another answer to the question could be that Greek political philosophy discovered and articulated the fundamental problems of political life with unsurpassed concreteness, clarity, and depth. Needless to say, Holmes rejects this answer. Without explaining the perennial interest in Greek philosophy, he encourages us to practice "liberal 'forgetfulness' toward Greece" (pp. 117, 127), as if the "diffuse rancor against modernity" (p. 117)—and who knows what other problems of modern times as well—would disappear if only we could get rid of that troublesome remembrance of the *polis* transmitted by Greek political philosophy.

I have quoted Strauss' statement on the relation of classical political philosophy to modern society at greater length than Holmes does in his footnote 9 (p. 117), because it concisely distinguishes three levels of political thought: philosophic principles, analysis of a given society, and immediately applicable prescriptions for political action. For brevity of expression, let us simply distinguish the level of fundamental philosophic principles from the level of description of the present and prescription for action. Political philosophy itself (and not only Greek political philosophy) stands or falls with this distinction. The claim of political philosophy is that the fullest possible understanding of any given society and the most reasonable possible prescriptions for action can be found only in the light of fundamental philosophic principles, developed on the basis of a comprehensive inquiry into human nature, human needs, and the means by which the political community can most fully satisfy those needs. Only philosophical reasoning can lead one beyond the limited opinions dominant in one's time and circumstances to the capacity for independent judgment of superior truth.

Holmes fails either to make this distinction or to argue that it cannot be made. This failure vitiates the point of the whole article. If fundamental principles of political philosophy are indefensible as a distinct level of thought, Greek political philosophy, which obviously is neither an analysis of contemporary society nor a set of immediately applicable prescriptions, would be patently obsolete, and this article would be unnecessary. If, on the other hand, this distinction is defensible, the argument of Holmes' article misses its target; relying simply on differences between contemporary society and the Greek *polis,* it does not address the issue of the adequacy of the fundamental principles of Greek political philosophy on the level on which they are presented.

II

Turning to Holmes' argument that a totalitarian potential inheres in Platonic and Aristotelian principles, one is first faced with the question, to whom is this argument addressed? Since Holmes sensibly differs from Karl Popper in not discerning "totalitarianism in the texts of the *Republic,* the *Laws* and the *Politics,*" he concedes that the danger arises only from "the imprudent importation of doctrines there espoused into the context of a highly differentiated modern society" (p. 118). I infer from this that his warning is not addressed to serious students of ancient political philosophy, since Aristotle, for example, teaches nothing if not sober prudence and moderation in politics. Indeed, I think one can safely rely on almost all political scientists to have sufficient prudence not to be carried to totalitarian excesses by an immoderate desire immediately to apply ancient Greek principles to modern society. Perhaps Holmes' argument, then, is directed to our politicians and politically influential intellectuals. But apart from the unlikelihood of their turning to an *American Political Science Review* article for guidance, surely one would be hard put to find any evidence of their being under the sway of the enticing premises of Greek political thinking.

Holmes asserts that the relation between Greek political theory and modern totalitarianism is an "old and vexing question" (p. 118), a "difficult and constantly recurring problem" (p. 115). He presents no evidence, however, to confirm the hypothesis that the study of classical political philosophy has led anyone toward totalitarianism, or that it is on "an anachronistic nostalgia for the integrated and heavily politicized life of the Greek city-state" (p. 117) that totalitarianism relies for ideological legitimation. Quite to the contrary, the goals by which modern totalitarianism seeks to justify itself are of a specifically modern sort, altogether opposed to the teachings of ancient political philosophy. Let us take Marxism-Leninism as the most important contemporary example. The creation of a realm of complete freedom through the total overcoming of necessity, including what earlier thinkers considered natural limits; the ultimate abolition of politics as it has existed hitherto; above all, the definitive solution of all human problems after the final historical revolution—what could be further removed from Aristotle's conception of the good political community than these beliefs? And what could be more alien to the spirit of sober moderation that characterizes Aristotle's whole teaching than hope for a radically different future such as to justify brutal and murderous means directed to its attainment?

Holmes fears that the Greek idea of politics, in modern times, "only serves to overlegitimate a technically efficient bureaucratic agency with police powers" and "makes it nearly impossible to understand why citizens might want to resist the coercive encroachments of a hypertrophic state" (p. 113, n. 1). These are flimsy grounds for arousing a Greek political philosophy scare. No one familiar with this philosophy is likely to confuse bureaucratic encroachments with political action as presented by Aristotle. Thomas Hobbes, an opponent of this philosophy who knew his enemy well, attacked it for a reason diametrically opposed to Holmes': "By reading of these Greek, and Latine Authors, men from their childhood have gotten a habit (under a false shew of Liberty) of favoring tumults, and of licentious controlling the actions of their Soveraigns; and again of controlling those controllers, with the effusion of so much blood; as I think I may truly say, there was never any thing so deerly bought, as these Western parts have bought the learning of the Greek and Latin tongues" (Hobbes, 1968, pp. 267–68). Stated with a non-Hobbesian intent: Greek political philosophy has been a perennial source of resistance to despotism. It remains to be seen whether the sort of normative philosophy apparently envisaged by Holmes, one whose purview is limited to what some theory of social evolution consecrates, can provide a solid basis for the defense of freedom.

III

Ancient political philosophy consists above all in complicated, profound, nuanced inquiries into the character and requirements of the political community and their relation to human happiness. To support his thesis, Holmes extracts simple dogmatic statements from the contexts that give them their full meaning. Thus he can say that "Plato, it cannot be denied, was an enthusiast of political participation" (p. 118), even though Plato's Socrates says of the best men:

> They have seen sufficiently the madness of the many, and that no one who minds the business of the cities does anything healthy, to say it in a word, and that there is no ally with whom one could go to the aid of justice and be preserved. Rather–just like a human being who has fallen in with wild beasts and is neither willing to join them in doing injustice nor sufficient as one man to resist all the savage animals–one would perish before he has been of any use to city or friends and be of no profit to himself or others. Taking all this into the calculation, he keeps quiet and minds his own business–as a man in a storm, when dust and rain are blown about by the wind, stands aside under a little wall. Seeing others filled full of lawlessness, he is content if somehow he himself can live his life here pure of injustice and unholy deeds, and take his leave from it graciously and cheerfully with fair hope (*Republic, VI,* 496 c 7–e 2).[1]

This point is not a side issue, for the relation of the philosopher to the *polis*–explored in depth by both Plato and Aristotle–is crucial to the question of the degree to which politics can achieve the fullest human happiness and hence to the question of the true status of the *polis.*

The limits of a brief comment keep me from discussing many, let alone all, instances of what I take to be misleading simplifications of ancient thought contained in this article. Instead, I shall examine one Greek conception to which Holmes attributes central importance: that the political community is a whole, whose parts are the citizens. To this conception, Holmes makes his two basic objections: that it is clearly obsolete as a description of modern society, and that the attempt to think about modern society in its light has totalitarian consequences. I shall argue that fuller reflection on this Greek conception refutes both objections.

Holmes argues that the social differentiation of modern society, with the existence of many quasi-autonomous interaction contexts or channels of social communication, has caused the modern political arena to dwindle "to the lowly rank of a subsystem among subsystems" (p. 126, n. 46), so that the political community can no longer be reasonably thought of as a whole composed of citizen parts. To assign a merely equal rank to the political ignores the most necessary function of any political society, which is defense. In wartime, or when danger of war threatens, the wholeness of a political community asserts itself most powerfully. Those engaged in defending their country characteristically understand their action and sacrifice as directed to defending certain political and moral values and a certain way of life. Modern liberal societies do indeed leave many aspects of social life quasi-autonomous, but their quasi-autonomy is permitted and supported by political decisions, made in the context of a certain kind of political regime: a liberal democracy. A different regime–for example that of the Soviet Union–rests on different moral and political values, aims at a different way of life, has consequently different contexts of social interaction, and is therefore quite a different whole. According to Plato and Aristotle, it is principally the regime (*politeia*) that orders the political community and makes it a whole, and each regime tends to foster certain human types and a certain way of life.

Plato identified democracy as one such regime, which is characterized first of all by its honoring of freedom. Because of this freedom, one way of life is not univocally imposed by society; indeed, democracy is the most variegated regime, and one can find all types of people in it. Predominant in its way of life, however, is an attachment to freedom, a lack of compulsion to rule or be ruled, an equal openness to all kinds of desires and activities (*Republic, IX,* 557 b–563 d). With Plato's vivid caricature of democracy, compare Holmes' statements on the way of life in modern society in footnote 50 (p. 126): the rights of man and their structural correlates reinforced "human individuality at the intersection of several overlapping contexts of social communication. . . . In such a highly differentiated society, individuality begins to seem like an 'autogenic' achievement of single persons who, without any socially approved pattern to follow, manage to integrate their diverse roles into coherent life stories." Despite vast differences in social structures and many other things, the similarity is amazing. Though different in massive respects, modern liberal democracy and the democratic regime depicted by Plato foster fundamentally the same values and way of life. From this I conclude that the conception of political society as a whole, made whole chiefly by its regime, is not clearly obsolete.

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[1] Holmes' own footnote 14 (p. 118) admits that his statement needs qualification; this does not, however, lead him to modify his simplistic assertions.

Holmes takes the example of Aristippus as emblematic for his whole argument: whereas the apoliticism of Aristippus appears outrageous and perverse in the context of an ancient *polis,* apolitical ideals in the context of modern liberal society seem perfectly acceptable (p. 124). In arguing against Aristippus, Socrates does not appeal merely to the conventional opinions of his day—after all, from the standpoint of such opinions Socrates himself appeared outrageous and perverse.[2] Socrates' deeper point is that Aristippus has a superficial view of his relation to the political. Their conversation makes clear that Aristippus knows how to discipline his slaves; his possession of slaves highlights the dependency of his way of life on political institutions; Aristippus has not thought through the character and significance of his dependency on the *polis* (Xenophon, *Memorabilia,* II, 1, 16–17; see Strauss, 1972, pp. 34–35). Apolitical thinking in contemporary liberal society suffers from the same defect of superficiality; it fails to recognize the ultimate dependency of individual freedoms (and of the quasi-autonomy of social interaction contexts) on the political regime of liberal democracy. Fundamental moral and political convictions, together with political institutions appropriate to them, constitute this regime which makes liberal political society a whole; a centerless plurality of quasi-autonomous social communications channels could not long endure without such a regime.

Does this conception of political society as a whole tend toward totalitarianism? What I have said so far about the conception of the regime as making society a whole has no such tendency. Holmes' fear in this regard seems rather to stem from what Plato and Aristotle say about the best regime. Aristotle, for example, argues that the highest goal of the best *polis* is the full happiness of its citizens, and that this *polis* must therefore aim at forming citizens of the most complete virtue or excellence possible (not, for example, merely courageous citizens who have only this one part of virtue). If this were all one knew of Aristotle's teaching, one could imagine its leading an enthusiastic person wholly without experience of politics to conceive the mad project of transforming all members of a modern liberal democracy into fully virtuous citizens; if such a person *per impossibile* acquired great political power, I suppose that person would do great harm, which might include totalitarian measures. Aristotle's presentation of the best regime, however, is a necessary completion of his whole political science; it is not a prescription for rash actions in the wrong circumstances.

Aristotle acknowledges that the best regime does not exist: "It is true that, in most states, most of the laws are only a promiscuous heap of legislation; but we have to confess that where they are directed, in any degree, to a single object, that object is always conquest" (*Politics,* 1324b 5–8). Furthermore, the possibility of successfully instituting the best legislation presupposes that all the best conditions—geographical, climatological, and human—be already given by chance; one can only pray for them (1332a 28–32). The best regime will very rarely if ever be possible. Accordingly, Aristotle presents another, inferior regime—the polity or mixed regime—as the best of which most states will ever be capable. This regime aims at a much more ordinary level of virtue in its citizens; a certain military virtue (1279a 39–b 5), fairness and moderation, and law-abidingness (1295a 25–1296a 22). But a particular regime can succeed in a political community only if existing circumstances (such as the existing balance of social forces) are suitable; for instance, the mixed polity is appropriate only for a society with a sufficiently large middle class. Therefore even this regime is often impossible; according to Aristotle, "Nowadays, when states have become still larger, we may almost say that it is hardly even possible for any other form of *politeia* [than democracy] to exist" (1286b 20–23). According to Aristotle's teaching, one needs a clear conception of the simply best regime in order to make the soundest possible judgments concerning the broad range of better and worse actual political societies and their regimes. Political action must be guided by prudent deliberation on all the politically relevant circumstances; in practically all cases this entails the setting of goals that are much lower than those of the best regime, and in all cases this requires prudent adaptation of political measures to existing circumstances. The sober moderation taught by Aristotle could give no encouragement to tyranny in ancient Greece and can give none to totalitarian excesses today.

IV

What I find ultimately most unsatisfactory about Holmes' article is the view of normative political thinking that underlies his whole mode of arguing. His fundamental assertion, that social evolution has rendered Greek principles of political philosophy obsolete and irrational, depends on a subordination of philosophical reasoning to historical process. Unless one can then establish, as Hegel tried to do, a complete philosophy of history such as to provide rational assurance that the historical process is rational and progressive, one abandons the very possibility of political philosophy. That is, one surrenders the autonomy of reasoning about the fundamental questions with which political philosophy has been concerned; one restricts the scope of normative reasoning to what happens to prevail at a given time in the flux of historical evolution. I think that Holmes is aware of these consequences. He allows that "verbally, of course, it is possible to defend the 'timeless validity' of the works of Plato and Aristotle" (p. 113); but goes on to assert that these ancient ideas must be discarded if they cannot be directly applied to modern society. Timelessly valid ideas, however defensible in verbal argument, are refuted by the course of historical development; philosophical reasoning is overthrown.

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[2] Holmes himself indicates this in footnote 44 (p. 125), but without thinking through its implications.

It is this subjection of normative reasoning to historical process, rather than ancient Greek principles, that runs the risk of tending toward totalitarianism. At a time when liberal democracies may seem relatively strong, one might entertain the comforting thought that the abandonment of fundamental philosophic independence will only lend support to rather good existing circumstances, by discouraging us from disquieting reflections on political alternatives that are perhaps superior. But a grave danger lies hidden in thus mortgaging one's independence of philosophic thought to the uncertain future, for who knows what fearful or contemptible human arrangements may not be brought forth and favored by the unpredictable processes of historical change? It is for this reason, and not from some irrational nostalgia for the Greek *polis,* that profound thinkers of our century have proposed the serious reconsideration of ancient political philosophy.

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A Reply to Nichols

STEPHEN TAYLOR HOLMES
Harvard University

James Nichols' comment, as I understand it, consists of six interrelated objections. Two of them appear mutually inconsistent, since Nichols seems to maintain both

(1) that an account of the long-acknowledged dissimilarity between modern society and the ancient city which relies primarily on the concept "structural differentiation" is trivially true and does not require stating (Nichols, 1979, p. 129),

as well as

(2) that the same account is most certainly false because the modern polity can still be adequately described as a "whole" made out of "parts" (p. 131).

In addition to making these two incompatible assertions, Nichols also alleges

(3) that theoretical proposals which encourage cooperation between political philosophy and political sociology are not only inherently futile, but also compromise the "autonomy" of philosophy and thus make it impossible to give a principled defense of freedom (pp. 13 0, 131, 133);

(4) that Aristotle's unrivaled "sobriety" as well as his explicit warnings against attempts to realize "man's essence" in the wrong contexts, make it highly objectionable to interpret his theory of the *polis* as in any way progenitive of the ideology of the total state (pp. 130, 132);

(5) that no evidence is presented to show that the "critique" of bourgeois society on the basis of pre-bourgeois ideals has ever contributed to the legitimation of modern tyranny (p. 130);

and finally,

(6) that it is possible to perceive a distorted echo of ancient liberty within totalitarian movements only by underestimating the decisive importance for the ancients of the contemplative life, the life reserved for that tiny handful of "the best men," who (too good for this world) expect next to nothing from politics and huddle "under a little wall" (p. 131).

While these claims are not all equally arbitrary, none of them seems particularly weighty or convincing. Taken in conjunction, as I will now try to show, they reveal Nichols' failure to understand what is at stake in my argument.

Objection One. Nichols' gentle irony about the "obviousness" of the structural dissimilarity between modern society and the ancient city has a strategic function. It is meant to conceal the essentially controversial character of all general accounts of this dissimilarity. This, in turn, allows Nichols to play down the insufficiency of *stating that* ancient political principles are not immediately applicable to modern society. Contrary to what he suggests, it is necessary to supplement this sort of conventional recognition by *analyzing why* (and to what extent) these principles have become inapplicable.

Objection Two. When he turns from irony to argument, in fact, Nichols admits the disputable and hence nontrivial character of my analysis (p. 131). He counters the suggestion that the structural differentiation of society has undermined the descriptive adequacy of Aristotle's whole/part schematization of the polity with the empirical claim that modern states can still be successfully analyzed as "wholes" made out of "parts." Although it is heartening to see Nichols make an empirical claim, it is disappointing that he makes such an implausible one.

The complicated *interdependence* between the polity and other sectors of modern society[1] does nothing to justify the wildly anachronistic notion that the political sphere can still be aptly depicted as a "totality" which contains individuals as subordinate "elements." To understand what is involved in the whole/part schema, it is helpful to recall Aristotle's striking comparison between the city and a chorus (*Politics,* 1276b 1–6). If a chorus switches "regime," so says Aristotle, if it changes from a tragic to a comic chorus, then (even if the individual singers remain the same) the group *as a whole* is utterly transformed. Applied to the city, this means that politics is such an overriding and dominant factor that it is essentially meaningless to distinguish between society as a whole and the political regime.

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[1] Of course, there is nothing in the classical concept of social differentiation to suggest that society could "long endure" (p. 132) without the polity, since independence and interdependence are always said to increase simultaneously (Durkheim, 1964, pp. 37 ff. and Simmel, 1978, pp. 295 ff.). Just so, Nichols' assertion (p. 132) that I conceive the political system as "equal" to all other sectors of society is arbitrary and based on nothing in the text.

In order to convince Nichols that he cannot possibly mean to "apply" these ideas to modern politics, it should suffice to point out that in attempting to do so he would have to deny the descriptive correctness of the state/society distinction. As an example, one might consider the fact that both Spain and Greece have recently acquired different regimes (rather than having merely experienced turnovers of personnel). But would anyone want to claim that they are now *different societies* in the totalizing Aristotelian sense?

Contrary to what Nichols suggests, moreover, there is a vast difference between the *ways* in which a constitutional-pluralist regime and a totalitarian regime characterize or influence their respective societies. One of the basic ideas of the bourgeois-liberal tradition is that individual liberty depends upon the *successful functioning* of society, on the capacity of various nonpolitical institutions to create those possibilities or "communication chances" which are the preconditions for freedom or meaningful choice. Unlike Aristotle's *politeia,* the *Rechtsstaat* was never meant to create these possibilities, but merely to help protect them once they had been created within diverse sectors of society. Thus a constitutional regime "characterizes" its society precisely by being self-limiting. Those who succeed in multiparty politics, for example, are constrained to recognize that there are many things *beyond* politics. This is why the modern polity cannot be adequately conceived either as a "whole" encapsulating its subordinate "parts," or (to follow Nichols' reformulation) as a command center or Archimedean point which "orders" society (p. 131) and makes it cohere. The fact that total regimes are often represented as the ordering "centers" of their societies, on the other hand, may well be the symptom of a deplorable *morbus Graecus.* But it certainly does not support the idea that the whole/part schema is generally applicable to modern political systems.

As an aside meant to clinch his case, Nichols adds that modern states must be "wholes" made out of "parts" in order to go to war (p. 131). He is right, of course, if he means to say that nineteenth-century liberals such as Constant were overly optimistic in their conviction that *international* social systems—especially commerce and science—would undermine patriotic solidarity to the extent of making impossible armed conflict between *national* political units. No doubt about it, political decisions leading to war between territorial nation-states have successfully disrupted various worldwide channels of social communication. But the very fact that disruption has been involved, shows how gratuitous it would be to affirm the all-englobing nature of politics on the grounds that the calamity of war (in contrast to peace!) brings out the "essence" of society.[2]

Objection Three. Nichols is convinced, even without looking, that all fundamental political principles can be made fully intelligible by philosophical commentaries on immutable human nature. Unfortunately, the fundamental principles of modern liberalism (the bourgeois "rights of man") *cannot* be made fully intelligible without explicit reference to the highly differentiated institutional order of modern "bourgeois" society. Bourgeois rights are valued neither because they mysteriously "inhere" in transhistorical *natura humana* nor because they "reflect" our innermost essence, but rather because they help solve a specific social problem: they make a significant contribution to maintaining and protecting social differentiation. (This point is brilliantly argued in Luhmann, 1965.) That is to say, modern constitutional rights help ward off the novel threat of "dedifferentiation" posed by the expansionistic or "imperialistic" tendencies of the modern political sphere, the danger that political authorities will cancel the newly achieved autonomy of multiple channels of social communication such as religion and economy.[3] In other words, what is protected by bourgeois rights is neither the "pre-social individual" nor "indestructible human nature" but rather a relatively fragile communicative order. As a consequence, these rights cannot be adequately understood within the conceptual universe of Greek political theory, no matter to what extremes ancient philosophers may have pursued their "ahistoricism." According to this argument, of course, the striking dissimilarity between the fundamental principles of ancient and modern political thought has nothing to do with any modern pretence to superiority over the ancients. Instead, it stems from the fact (does Nichols deny it?) that the relatively undifferentiated character of the ancient city would have made it absurd to conceive the polity as an expansionistic "subsystem" which needed to be obstructed from eroding the differentiation of society, that is to say, from repoliticizing religion, economy, art, law, sport and so forth.[4]

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[2] For the classic statement of the idea that war brings out the "essence" of society, see Schmitt (1976).

[3] The ancient claim that the sovereign is *legibus solutus* seems to have become a conspicuous menace precisely when the "state" shed the moderating influence of diffuse moral, religious and familial constraints characterizing the previous "embeddedness" of political authority in a relatively undifferentiated feudal order. Thus, as Eisenstadt has argued, the threat of *de*differentiation is a consequence of differentiation itself: "The growing differentiation and increasing interdependence among various more autonomous and diversified institutional spheres increases the probability that one sphere will attempt to dominate the others coercively, by restricting their tendencies toward autonomy. This probability is especially strong with respect to the political and religious . . . spheres, because these

To understand why Nichols finds all cooperation between normative political philosophy and descriptive social theory so distasteful, it is necessary to attend to his weakness for melodramatic disjunctions: foundationalism *or* nihilism; the tribunal of reason *or* the "flux" of history; ancient moderation *or* modern hubris; certainty *or* directionlessness; pure philosophy *or* no philosophy. Because he accepts the inevitability of such theatrical choices, Nichols is forced to cast anyone skeptical about a (non-vague) human nature into the disreputable role of an unhinged fanatic. That this is an unearned triumph is the least that might be said.

Pure or anti-sociological philosophy cannot explain freedom, because freedom is freedom in society and not freedom from society. Just so, pure or ahistorical "norms" cannot provide a "solid basis for the defense of freedom" (p. 131). If all institutions collapsed and the possibility of organized political action were totally undermined, then the defense of freedom would be (regrettably) hopeless. Indeed, it is a quite banal insight that neither reciting homilies about human nature nor admiring Aristotle's sobriety would rescue such a desperate situation. Nichols knows this. It is only his prior commitment to "either/or reasoning" which prevents him from admitting how deeply normative principles and describable social structures are implicated in one another. But it is precisely this co-implication which explains the contribution which empirically oriented social theory can make to political partisanship. It is surely relevant here that ideologues of the total state often have more difficulty dealing with historical facts (such as how capitalism actually *has* developed) than with our purified principles, appeals to eternal human nature or moralizing *autos-da-fe.*

Objection Four. The entire structure of my argument was based on the premise that totalitarian autocracy is a *specifically modern development* (having to do with the coercive endeavor to "dedifferentiate" highly differentiated societies). Thus it is quite obscure why Nichols believes himself to be arguing *against* me when he informs us that Aristotle was a moderate man (pp. 130 and 133). Such an assertion is conspicuously irrelevant to the idea (explicitly formulated by Benjamin Constant) that the drastic structural transformations accompanying the modernization of Europe have created a novel rhetorical opportunity: the possibility of defending tyranny *in the name of* freedom and democracy. In fact, to prove that anachronistic Aristotelianism could never, in principle, play a role in legitimating tyrannous regimes, Nichols would have to make the textually unsupportable claim that Aristotle's "sobriety" prevented him from identifying the polity with total society.

Objection Five. Conscious that numerous modern liberals (such as Madison, Mill and even Constant) have explicitly acknowledged their partial debt to the political traditions of antiquity, Nichols is disturbed by my failure to provide equally specific historical evidence that modern appeals to the *res publica* conception of politics have also contributed to the philosophical legitimation of "total" regimes. Constant, of course, used the Abbé de Mably as

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spheres are especially prone to 'totalistic' orientations" (Eisenstadt, 1964, p. 381). Similarly, one could view the inherently expansionist character of the capitalist economy (which also followed upon its "emancipation" from the surrounding social nexus) as a menace to the autonomy of other contexts of interaction. Unemployment compensation, interpreted in the light of this idea, would have the exemplary value of social justice because it contributes, for example, to the maintenance of a differentiation between economic and family life. It helps prohibit every fluctuation of the economy from penetrating and thereby devastating an individual's life outside the workplace.

[4] I therefore cannot agree with Nichols that Plato presents a "vivid caricature" (p. 131) of modern liberal democracy (though he may have ridiculed all forms of ancient democracy), because Plato did not contemplate a "way of life" based on the specifically modern form of non-identity between polity and society.

his prime example of how "modern imitators of ancient republics" could try to justify tyranny in the name of freedom (Constant, 1957, pp. 1014–18). I, in turn, will limit myself to a brief and necessarily anecdotal remark about Giovanni Gentile, probably the most brilliant of the Italian intellectuals associated with fascism. It is wholly characteristic of Gentile's political thinking that he explains the original appeal of fascism in the following way.[5] Upon demobilization, he recounts, the Italian soldiers who had fought in the First World War returned home, still in uniform, "their eyes glistening" from the memory of camaraderie and risorgimento patriotism among the troops. What a shock they received when they returned to discover a "new" Italy, industrialized by the war effort, where economic competition and impersonal-instrumental relations had nearly quenched the old communal "feel" of society. So, Gentile continues, there was nothing more natural than for those young men to retain their uniforms and set about recasting Italian society in a familial mode (with a charismatic *pater patriae*), nothing more natural, in other words, than for them to try to "cancel" the loss of morality, the mean-spirited instrumental relations of the market or "privatized sphere" through the higher morality of the *res publica.* His whole mode of arguing, in sum, was based on an explicit echo of Aristotle. The failings of *poiēsis* were to be made good in the realm of *praxis.* Indeed, Gentile's legitimation of the fascist "ethical state" depended on his undisguised identification of politics with the dimension of "genuine intersubjectivity" or "the partnership in the good life," that is to say, with the moral sphere par excellence.

As Nichols might now point out, of course, the charge of anachronistic Aristotelianism is harder to make convincing in the case of the Marxist tradition than in the case of fascists like Gentile, even though the equal importance of Hegel's "critique" of bourgeois society for the young Marx is now a commonplace. The interest of pursuing my hypothesis in this direction as well, however, can be underscored by the recollection that in the most extreme phases of communist rule engineers and bureaucrats who made *technical* mistakes have been punished for *political* sabotage. The seemingly unlimited elasticity of the Stalinist category "crime against the state" reveals the logic of coercive dedifferentiation, of the retrogressive "identification" of the polity with total society.

Objection Six. In trying to answer Nichols' final criticism, I am forced to confront his perplexing combination of two seemingly disrelated concepts of the "autonomy of philosophy." The first may be called the "activist" or "foundationalist" conception, while the second may be referred to as "autonomy-as-withdrawal." When thinking primarily of the first concept, Nichols asserts that by keeping philosophy on a distinct "level" (p. 130), unflustered by historical information, he can furnish "a solid basis for the defense of freedom" (p. 131). By securing a purchase on eternal human nature, he suggests, philosophers will be able to act effectively in the world, "come what may." To pursue an alliance between philosophy and sociology, by contrast, involves "mortgaging one's independence of philosophical thought to an uncertain future" (p. 133) and promising to accommodate oneself ignominiously to every atrocity that happens along.

Although this is one of Nichols' main arguments, it collapses at first touch. Nichols even admits that philosophy, detached from institutions and organized political action, cannot provide a "solid basis" for the defense of freedom. That is why he cites with approval Plato's justification for high-minded philosophers to withdraw from this grubby world *and do nothing* (except "save" themselves) when things get bad enough. By half-interpreting the autonomy of philosophy as the aloofness of philosophers, Nichols reveals the baselessness of his own protest. Although he says, for example, that Socrates' criticism of Aristippus is not the conventional or collectivist one I present, when he proceeds to specify Socrates' "deeper point" (p. 132), he lapses into a repetition of my own reading of Xenophon's quite conventional claim that Aristippus is a "part" who does not recognize his actual dependence on the social "whole." I interpret this failure to fulfill the expectations he arouses as a tacit admission that philosophers who retreat under little walls cannot provide an "autonomous foundation" for politics or political action at all.

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[5] Gentile (1924, pp. 171–77; see also Gentile, 1961; pp. 103–20). I should also refer here to the excellent article of Manfred Riedel (1963) concerned with the anachronistic Aristotelianism of nationalistic German historiography in the nineteenth century.

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The Good, the Beautiful, and the Useful: Montaigne's Transvaluation of Values*

DAVID LEWIS SCHAEFER
Holy Cross College

Historians of political thought have failed to appreciate the importance of Montaigne's Essays *as a major work of modern political philosophy. The neglect of the* Essays *as a political book is traceable to a widespread belief that Montaigne's thought is too fluctuating and unsystematic to embody a coherent and profound political teaching.*

This study challenges the prevailing interpretation of Montaigne's thought by analyzing his critique of the classical understanding of supreme or "heroic" moral virtue, concentrating especially on the chapter entitled "Of Cruelty." A careful reading of this chapter reveals that underlying the seeming disunity or inconsistency of its argument is a carefully worked out political intention. The essence of Montaigne's political project involves the replacement of a traditional morality based on "beauty" (i.e., one in which humanity aspires to share in the divine) by one embodying "utility" (one in which we understand our needs in the light of what we share with the beasts). The analysis of this "transvaluation of values" helps one to comprehend the foundations of the "bourgeois" morality that characterizes modern liberal regimes.

Despite its extensive treatment of political and moral issues, Montaigne's *Essays*, one of the most popular and influential books ever written in French, has been seriously neglected by students of political philosophy.[1] This neglect is traceable in part to a widespread belief that Montaigne's thought is too fluctuating and unsystematic to embody a coherent and profound political teaching. Even those scholars who have examined Montaigne's thought about politics have generally found it to embody a multitude of conflicting opinions and attitudes, such as a radically skeptical posture towards accepted laws and customs combined with an extreme practical conservatism.[2] And the most popular twentieth-century interpretation of the *Essays* as a whole views it as the record of a sweeping "evolution" Montaigne underwent from a deferential admiration of classical learning and Stoic virtue, through a "skeptical crisis," to an "Epicurean" attitude, embodied particularly in Book III, of tolerant hedonism.[3]

I wish to challenge the prevailing interpretation of Montaigne's thought by analyzing his treatment of the classical understanding of supreme or "heroic" moral

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[1] All references to Montaigne's works will be to the Thibaudet-Rat edition (1962); however, I have usually followed the translation by Frame (1957). Citations of the *Essays* within the body of the text will be included in parentheses; the three numbers refer successively to volume, chapter, and page.

[2] Cf. for instance, Brown (1963), especially the claim (p. 83) that the essayist made an "error in judgment" in this regard. Sayce (1972) finds a conflict between Montaigne's conservative "temperament and personality" as expressed in his book and "his influence and the later history of his subversive ideas" (p. 259). Keohane (1977) holds that the essayist's political thought culminates in a series of "tensions," e.g., between freedom and restraint (p. 369), regarding the power of reason (p. 372), concerning the adequacy of custom as a source for standards (p. 374), and between autonomy and authority (p. 379). Like Sayce, Keohane finds a contradiction between the intended political effect of the *Essays* and its actual one (p. 386).

Although only Brown says so explicitly, each of the above three interpretations, if correct, would compel one to doubt Montaigne's political sagacity. It would hardly take much profundity to know, for instance, that the goal Keohane attributes to Montaigne of teaching humanity "that we must uphold established ways of doing things, and obey authority" is not likely to be advanced by publishing "a book which probes and questions everything" (Keohane, p. 380). And we have Montaigne's own testimony to the contrary: "Obedience is not pure or tranquil in a man who reasons and argues" (II, xvii, 640).

[3] The most influential work setting forth this interpretation is Villey (1908).

virtue, concentrating on one chapter, "Of Cruelty" (II, xi), in which that treatment is most thematic. The chapter as a whole exemplifies the manner in which Montaigne, throughout the *Essays,* endeavors to challenge the classical understanding of morality and to replace it with a less demanding and consequently more "humane" view of humanity. By penetrating Montaigne's rhetorical treatment of his subject, I wish to suggest that his purported "evolution" conceals a profound and fully consistent political teaching.[4] I shall contend that this teaching embodies an attempt to revolutionize our understanding of morality and its bearing on politics. The intellectual evolution that Montaigne intended has borne fruit in the secularist, egalitarian politics of modernity; and anyone who wishes to appreciate both the rationale and the problem of modern politics would do well, therefore, to consider Montaigne's argument with care.

"Of Cruelty": First Reading

As in the case with many of Montaigne's chapters,[5] the reason for the title "Of Cruelty" is at first obscure. The essayist begins by setting forth a distinction between "virtue" strictly understood and "the inclinations towards goodness that are born in us." Even though souls that are "self-regulated and well born follow the same path, and show the same countenance in their actions, as virtuous ones," virtue nonetheless "means something greater and more active than letting oneself, by a happy disposition, be led gently and peacefully in the footsteps of reason. . . . For it seems that the name of virtue presupposes difficulty and contrast, and that it cannot be exercised without opposition." Indeed, since the truly virtuous human being must overcome natural inclinations to the contrary, such an individual, Montaigne suggests, exercises through self-struggle an effort that surpasses that of God.[6] Among the examples Montaigne cites of those who strove to practice such virtue are many "Stoic and Epicurean philosophers," who "seek pain, need, and contempt in order to combat them and to keep their soul in trim." (II, xi, 400–01).

Having set forth this account of virtue, Montaigne immediately discovers that it embodies certain difficulties. In the first place, although the essayist holds "the soul of Socrates [to be] the most perfect that has come to my knowledge," by the criterion of virtue thus understood it would be a soul deserving little commendation, since it is impossible to conceive any "vicious lust" even being present to oppose Socrates' all-powerful reason (II, xi, 402). There are other difficulties as well:

> If virtue can shine only by clashing with opposing appetites, shall we say then that it cannot do without the assistance of vice, and that it owes to vice its credit and honor? What would become also of that brave and generous Epicurean voluptuousness, which undertakes to bring up virtue softly in her bosom and make it frolic, giving it as its playthings shame, fever, poverty, death, and tortures? If I presuppose that perfect virtue makes itself known in combating pain and bearing it patiently, . . . if I give it unpleasantness and difficulty as its object; what will become of the virtue that has climbed so high that it not only despises pain but rejoices in it and feels tickled by the pangs of a bad colic, the kind of virtue established by the Epicureans, of which many of them by their actions have left us most certain proofs? As have many others, who I find have surpassed in practice the very rules of their discipline (II, xi, 402–03).

The problem Montaigne has uncovered in the understanding of virtue as necessarily involving struggle, he implies, points to a difficulty or self-contradiction embodied in the Epicurean doctrine, and seemingly (indeed, more manifestly) in Stoicism as well.[7] He has previously suggested that this difficulty is inherent in the prescription of all those "spiritual and bodily physicians" who "as if by a conspiracy,

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[4] For a detailed critique of the "evolutionary" interpretation of the *Essays,* as well as an exposition of Montaigne's mode and purpose of writing, see Schaefer (1975).

[5] Cf. Montaigne's remark that "the titles of my chapters do not always embrace their matter; often they denote it only by some sign" (III, ix, 973).

[6] This argument is made more explicitly at II, xxix, 683; it follows Seneca, *Of Providence,* VI.

[7] Paradoxically, Montaigne treats the doctrines of Stoicism and Epicureanism as essentially similar, expressly rejecting the "common opinion" that regards the precepts of Stoicism as much firmer and more rigorous than those of Epicureanism (II, xi, 401). Thile he accuses others of having distorted Epicurus' thought in order to make his doctrine appear less demanding, Montaigne himself, in portraying Epicureanism as a doctrine that requires that one seek out occasions of pain, appears to be guilty of such distortion; see II, ii, 329, with the comment on this passage by Zeitlin (1935, Vol. II, p. 444).

The explanation of this historical distortion on Montaigne's part, I believe, is that from the point of view of the kind of libertarian hedonism that Monfind . . . no remedy for the diseases of the body and the soul, but by torment, misery, and pain" (I, xxx, 198). In order to excel, people are taught to seek out and endure unpleasantness; but if they succeed so well that unpleasantness itself becomes pleasurable, wherein lies their excellence? As an exemplification of this problem in "Of Cruelty," Montaigne cites the case of Cato the Younger, whom he elsewhere allows to "suffice for every example of virtue" (II, xiii, 594):

> When I see him dying and tearing out his entrails, I cannot be content to believe simply that he then had his soul totally free from disturbance and fright; I cannot believe that he merely maintained himself in the attitude that the rules of the Stoic sect ordained for him, sedate, without emotion, and impassible; there was, it seems to me, in that man's virtue too much lustiness and verdancy to stop there. I believe without a doubt that he felt pleasure and voluptuousness in so noble an action, and that he enjoyed himself more in it than in any other action of his life. . . . I seem to read in that action I know not what rejoicing of his soul, and an emotion of extraordinary pleasure and manly voluptuousness, when it considered the nobility and sublimity of its enterprise (II, xi, 403).

In the light of the example of Socrates as well as that of Cato, Montaigne therefore proceeds to revise his original account of virtue. He asserts that it is doubtless

> finer [*plus beau*] to prevent the birth of temptations by a lofty and divine resolution, and to have so formed oneself to virtue that the very seeds of the vices are rooted out [as Socrates and Cato appear to have done], than to prevent their progress by main force, and, having let oneself be surprised by the first commotion of the passions, to arm and tense oneself to stop their course and conquer them; and that this second action is still finer [*plus beau*] than simply to be provided with an easy and affable nature and having an innate distaste for debauchery and vice. For it certainly seems that this third and last way makes a man innocent, but not virtuous; exempt from doing ill, but not apt enough to do good. Besides, this condition is so close to imperfection and weakness that I do not very well know how to separate their confines and distinguish them (II, xi, 405).

Seemingly in order to justify this ranking, Montaigne observes that "the very names of goodness and innocence are for this reason terms of contempt." He goes on to note, however, "that several virtues, like chastity, sobriety, and temperance, can come to us through bodily failing," and that "firmness in dangers (if firmness it should be called), contempt for death, endurance in misfortunes, can come to men, and are often found in them, through failure to judge such accidents rightly, and not conceiving them as they are" (II, xi, 405). If Montaigne's aim is indeed to demonstrate the inferiority of "goodness" to "virtue," he seems to have proved too much: not only innocence, but also what people take to be virtue, may be the result of an imperfection.

At this point, Montaigne redirects his attention "in order to say a word about [himself]." He places himself at the lowest of the three ranks of excellence: he neither possesses habitual virtue, nor has he, in his words, "put myself to great effort to curb the desires by which I have found myself pressed." He possesses only a "virtue, or, to put it better, innocence, that is accidental and fortuitous," and that results from the mildness of his natural disposition. Although Montaigne "hold[s] most vices in horror," his freedom from vice is due "more to my fortune than to my reason," in that his "reasonings, having in some things broken away from the common road, would easily give me license for actions that this natural inclination makes me hate." He cites the cases of two other philosophers, Aristippus and Epicurus, who similarly displayed more restraint in their actions than in their opinions (406–07).

Montaigne continues his confession by describing the "softness" embodied in his distaste for cruelty, which he "cruelly hate[s], both by nature and by judgment, as the extreme of all vices." He adds that the pleasure which other people find in hunting is a more unruly passion than physical pleasure, although "those who have to combat" the latter argue that "at its greatest pitch it masters us to such an extent

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taigne ultimately advocates, the differences between Stoicism and Epicureanism appear much less significant than their similarities: in admonishing us to moderate our appetites, both doctrines (as well as the teachings both of classical philosophy generally and of Christianity) are far too rigorous.

It is doubtful that either the Stoics or the Epicureans are the major object of Montaigne's concern, in "Of Cruelty" or elsewhere. The essayist's attribution of the remark that "to philosophize is to learn how to die" to Cicero rather than to Socrates (I, xx, 79) suggests that he may use the Stoics and Epicureans as "stand-ins" for more serious antagonists, the classical political philosophers, just as Raymond Sebond serves as a stand-in for St. Thomas Aquinas (II, xii, 417). The real issue between Montaigne and the classical political philosophers, as the essayist indicates at III, ix, 967–68, is not the desirability of physical pleasure, but the necessity of teaching self-restraint and endurance to the multitude.

that reason can have no access."[8] As for himself, Montaigne professes to be unable to "see a chicken's neck wrung without distress" or "bear to hear the scream of a hare in the teeth of my dogs" (408–09).

Montaigne relates his abhorrence of cruelty to his sympathy for others' suffering, a sympathy that would readily cause him to weep "if I could weep for any occasion whatever." Having expressed his shock at the torments and persecutions that some people inflict on others, he explains how a "Latin author" (Suetonius) indirectly indicated his own horror at the cruelty practiced by "the Roman tyrants." And, in a passage that incurred the censure of some latter-day rulers of Rome, the Church authorities,[9] Montaigne objects to torture by remarking that "even in justice, all that goes beyond plain death seems to me pure cruelty, and especially for us who ought to have some concern about sending souls away in a good state; which cannot happen when we have agitated them and made them desperate by unbearable tortures." After recommending the abolition of this practice and the adoption of a more humane substitute for it, the essayist goes on to remark the frequency of cruelty that results from "the license of our civil wars"—wars rooted in the religious conflict between Catholics and Protestants. And, noting how the Romans progressed from "the spectacle of the slaughter of animals . . . to that of men," he infers that "nature herself . . . attaches to man some instinct for inhumanity" (II, xi, 410–12).

Having previously observed how the wars fought by Christians in the name of their religion promote inhumanity towards human beings, Montaigne now observes that piety ought to encourage gentleness even towards other animals, thus justifying his own sympathy for them:

> Theology herself orders us to show some favor in their regard; and considering that one and the same master has lodged us in this place for his service, and that they, like ourselves, are of his family, she is right to enjoin upon us some respect and affection toward them (II, xi, 412).

The theological teaching expressed in this quotation conforms to an argument by the Christian apologist Raymond Sebond, in a book which Montaigne purports to "defend" in the following chapter.[10] The essayist chooses, however, not to dwell on the alleged conduciveness of Christianity to sympathy for the animals, but refers instead to *pagan* theology to support his position. He cites the belief of various peoples in the doctrine of metempsychosis, and notes that "many nations, and notably some of the most ancient and noble, have not only received beasts into their society and company, but given them a rank far above themselves," some even deifying animals. Although Montaigne disclaims any belief in the "cousinship between us and the beasts," he proceeds to remark the "close resemblance" between us and them. He then introduces one of the central themes of the "Apology for Raymond Sebond," his most extensive critique of classical philosophy and revealed religion, by professing to "beat down a lot of our presumption" and to "willingly resign that imaginary kingship that people give us over the other creatures" (cf. II, xii, 425–27). He asserts the existence of "a general duty of humanity, that attaches us not only to the beasts, who have life and feeling, but even to trees and plants." Montaigne concludes the chapter by recounting several stories of honors and services bestowed on animals by various ancient peoples and the contemporary Turks (II, xi, 413–16).

The Critique of Heroic Virtue

Let us look back over "Of Cruelty." Surveyed as a whole, the chapter seems to be divided into two separate parts, only tangentially related: a discussion of virtue, and a discussion of cruelty, with particular reference to the animals.[11] The key to understanding the chapter, however, lies in grasping the integral relation between the two parts.

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[8] Montaigne's argument here is in effect a critique of Aristotle's claim that anger or spiritedness (*thumos*) is more rational than appetite (*Nicomachean Ethics*, 1149a 25–b 4); cf. also the more radical claim by Socrates in Plato's *Republic* (440b) that anger never acts against the dictates of reason, a claim that is refuted by the very example that Socrates uses to demonstrate it (see Bloom, 1968, pp. 375–76). The question of whether it is more important to accommodate as opposed to subduing the thumotic rather than the appetitive part of human nature is in a way the fundamental practical issue between Montaigne's moral teaching and that of the ancients.

[9] Montaigne (1962, *Journal de voyage en Italie*, pp. 1228–29).

[10] Cf. Montaigne (1962, p. 1542 [editors' note 5 to p. 412]).

[11] Zeitlin, for one, remarks the seeming disharmony between these two parts of the chapter (1935, Vol. II, pp. 474–75).

The relation of the parts becomes more evident when we consider the overall movement of the argument of the chapter from "high" to "low" things. "Of Cruelty" almost begins with a reference to the virtuous human being's surpassing God; it ends with a discussion of the "resemblance between us and the animals," and even trees and plants, that is intended to undermine humanity's claim to superiority to other living matter (let alone God). In between these two poles, the chapter moves from a seeming praise of "tough" virtue, to the observation that such virtue can frequently be a sham, to Montaigne's account of his own easygoing ways, to his admission of softness, and finally to his criticism of the cruelty that human beings display both towards each other and towards the beasts.

I suggest the following interpretation of this movement. By linking "hard" virtue to the quest to surpass or at least emulate God, and "soft" goodness to the recognition of humanity's kinship with the beasts, Montaigne wishes to indicate—as he elaborates in the "Apology for Raymond Sebond"—that it is more *salutary* to look at humanity in the light of the beasts than to relate the human species to a transcendent God.[12] Because people think they can rival God, they are driven to exercise a terrible cruelty towards themselves and others in the name of virtue.[13] Thinking of the human being, on the other hand, not as godlike, but merely as a creature possessing "life and feeling" in common with the other animals and even with trees and plants, is conducive to a mildness that Montaigne identifies as "humanity" (II, xi, 414). Paradoxically, to be humane towards one's fellow human beings seems to require not only that one recognize their susceptibility to suffering and hence their non-divine character, but also that this animal susceptibility be regarded as more important than the capacity to strive after the divine that makes them distinctively human.[14]

This interpretation of "Of Cruelty" is borne out when we reconsider Montaigne's portrait of Cato the Younger as a model of heroic virtue in the light of remarks found elsewhere in the *Essays*. As we have seen, after seeming to admire Cato's extreme courage in II, xi, Montaigne goes on to cast a shadow of doubt over that courage by remarking how easy it is for an outside observer to misjudge such qualities.[15] A similar hint of doubt is present in "Of Cato the Younger," in which Cato is presented as an example of "how far human virtue and firmness could go." Montaigne purports to denounce the corrupt judgments of the wits of his time, who use "their ingenuity to obscure the glory of the beautiful [*belles*] and noble actions of antiquity, giving them some vile interpretation and conjuring up vain occasions for them." Several causes compel one to doubt the sincerity of this denunciation. First, it may be significant that Montaigne, purporting to be satiric, places himself on the side of the unbelievers: "Give me the most excellent and purest action, and *I* will supply fifty vicious motives for it" (emphasis added). Second, he proceeds to carry out his offer in a way, by calling attention to the belief of some ancients that Cato's suicide was motivated by his fear of Caesar, or by ambition. Third, although he counters such criticisms by asserting that Cato "would actually have preferred to perform a beautiful [*beau*], noble, and just action with ignominy than for the sake of glory," he both precedes and follows this remark with the suggestion that the praise of a virtue such as Cato's may be the result of passion or a poetic frenzy that transcends reason (I, xxxvii, 226–28). Although Montaigne appears to endorse such enthusiasm here, it stands in marked contrast to his own endeavor, frequently remarked in the *Essays*, to judge human beings soberly (cf. II, xii, 545–53; III, xiii, 1051–53).

The doubt about the true cause of heroic virtue which Montaigne attributes to others in I, xxxvii, is explicitly endorsed by the essayist in "Of Drunkenness." Here he reports that Plutarch, "so perfect and excellent a judge of human actions, on seeing Brutus and Torquatus kill their children, came to doubt whether virtue could go that far, and to wonder whether those personages had not been motivated by some other passion." Montaigne himself adds that "all actions outside the ordinary limits are subject to sinister interpretation. . . ." He cites the Stoics' contempt for sensual pleasure, the Epicureans' extreme endurance, and the boasts of "our martyrs" as evincing a "runaway courage" akin to that which the heat of combat instills in "high-souled soldiers," or to poetic *mania.* He contrasts such conduct with the way of reason, which consists in "an orderly management of our soul" (II, ii, 329–30; cf. II, xxix, 683).

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[12] Cf. II, xii, 464–65. For an interpretation of the "Apology," see Schaefer (1979).

[13] The chapter "Of Virtue" is correspondingly filled with stories of the cruelty people have inflicted on themselves. Cf. Descartes (1953, p. 130): "Often what [the ancient writers] call by such a beautiful name [as that of virtue] is nothing but insensibility or pride, or despair, or parricide."

[14] Contrast Hegel's argument in defense of capital punishment, *The Philosophy of Right*, Pt. I, Sec. 100.

[15] This fact is indirectly acknowledged by Aristotle; see *Nicomachean Ethics*, III. 8, with Jaffa (1952, p. 56).

As Book II proceeds, Montaigne continues to question the motives that underlie virtue such as Cato's. At one point in the "Apology for Raymond Sebond" he observes "how imperceptibly near madness is to . . . the effects of a supreme and extraordinary virtue" (II, xii, 471–72). Later in the same chapter he remarks the "rashness" of philosophy's having taught "that men produce their greatest deeds and those most closely approaching divinity when they are out of their minds and enraged and mad."[16] After noting Cato's calmness (a quality not necessarily incompatible with madness) on the night before he killed himself, Montaigne questions the uniqueness of such firm resolution by citing the case of a woman who killed herself to escape the torments of her husband, having meditated on the plan overnight; and a whole order of men in an oriental nation who kill themselves after "constant premeditation through a whole life" (II, xxviii, 682; II, xxix, 685–86). In the same chapter–entitled "Of Virtue"–Montaigne recounts stories of men lesser known than Cato who cut off their sexual organs for various reasons and then remarks, "If this had been done through reason and religion, . . . what should we not say of so sublime an enterprise?" (II, xxix, 685–85.)

Each of these remarks leads the reader to wonder whether Cato's suicide, despite the "elevated" purpose that seemingly justified it, was not in fact motivated by the same self-destructive passion that shocks one in Montaigne's other examples. The resemblance between Cato's motivation and that of "lesser" human beings is further suggested in the chapter entitled "That Our Desire Is Increased by Difficulty." After remarking that "our will is whetted by opposition," and that "difficulty gives value to things," Montaigne observes: "that great Cato found himself, just like the rest of us, weary of his wife as long as she was his, and desired her when she belonged to another man." He immediately adds that a similar kind of attitude can be found among horses.[17]

Montaigne's account of Cato's attitudes towards his wife invites us to suspect that the same desire for difficulty lay at the root of the Roman's virtue. Just as Cato's wife lost her attraction when she was his, so other objects and activities that are easily available lacked savor for him. Cato was driven by a passion to distinguish himself from other men: to demonstrate that he had surpassed the ordinary human condition. But by asserting that Cato must have "enjoyed himself more" in his suicide "than in any other action of his life" (II, xi, 403), Montaigne causes one to doubt whether the Roman is really entitled to be admired for it. If Cato did what he did because it was pleasant to him, in what way was he truly different from other people who do more commonplace things for the same reason? And as for the original process of self-training that enabled Cato to find pleasure in what to others is most painful, this was simply the product of a compulsive desire for distinction that in itself represents no greater a motive than that which leads the glutton to overeat, or Don Juan to pursue an endless succession of conquests.

The Good, the Beautiful, and the Useful

Assuming the validity of Montaigne's reductionist explanation of Cato's own motivation, this explanation still does not account for why people admire Cato in a way they do not admire gluttons or even Don Juans. Montaigne traces this admiration, in part, to a natural human love of "beauty" that leads humanity to want to elevate itself above the merely animal condition, and to see itself as godlike, and its situation as more beautiful than it really is.

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[16] II, xii, 551. For one likely referent of this remark, see Plato, *Phaedrus,* 244–45.

[17] II, xv, 597. No such motivation is attributed to Cato by Plutarch. See Plutarch (pp. 932, 948); also, Norton (1906, p. 127). It should be added that Montaigne's entire disparaging account of Cato stands in marked contrast with Plutarch's emphasis on the Roman's virtue. On the other hand, the story of Cato's giving up his wife to his friend Hortensius as Plutarch tells it is rather astonishing, and seems to invite some sort of additional explanation, if not that suggested by Montaigne, then the one Plutarch attributes to Caesar (Plutarch, p. 948). And other points in the Plutarchean account of Cato's life do lend support to the criticism made by his contemporary Cicero, as quoted by Francis Bacon in *The Advancement of Learning,* I. 3, 5: "Cato means excellently well, but he does hurt sometimes to the commonwealth; for he talks as if it were Plato's republic that we are living in, and not the dung of Romulus." This criticism is echoed by Montaigne, as I indicate below.

Montaigne frequently uses the adjective *beau* in describing Cato's virtue. This word has several senses, including "beautiful," "fine," and "noble." A similar ambiguity, the essayist notes, exists in Greek, where "one and the same word . . . embraces the beautiful and the good."[18]

Montaigne suggests that this identification of the good with the beautiful has its roots in the most primitive human consciousness, remarking that "the first distinction that existed among men, and the first consideration that gave some men preeminence over others, was probably the advantage of beauty" (II, xvii, 623). Beauty, he asserts, "holds the first place in human relations; it . . . seduces and prepossesses our judgment with great authority and a wondrous impression." Montaigne further suggests that the admiration of beauty found its way from popular opinion into the teachings of the philosophers. Having noted the importance Plato attached to it (according to Diogenes Laertius), he attributes to Aristotle the opinions that "to the beautiful belongs the right to command" and that human beings of godlike beauty are entitled to veneration (III, xii, 1035–36).

Montaigne implicitly takes issue with his philosophic predecessors by noting the unreliability of a person's physiognomy as a guide to character, observing particularly the contrast between the beauty of Socrates' soul and the ugliness of his body (II, xii, 1034–36). Yet it would seem that Montaigne is being unfair to Plato and Aristotle: it is precisely from Plato that one learns of Socrates' underlying beauty, and it is to the beauty of the soul rather than of the body that Aristotle assigns the right of political preeminence (Aristotle, *Politics,* I. 5, 1254b 32–1255a 1).

Montaigne knows what he is doing. His treatment of physiognomy, and his misstatement of Aristotle's view, point towards the more serious issue between his teaching and that of the classical philosophers: the question of whether the beauty of the soul itself is a proper criterion for judging human conduct.

Neither Plato nor Aristotle contends that the good can simply be equated with the beautiful. It is evident that many things that seem beautiful may prove, upon reflection and analysis, to be harmful. Indeed, judgments of beauty are less reliable in ordinary life than judgments of utility are, since the former (as Stanley Rosen points out in his summary of the classical understanding on this point) are "more closely conditioned by the body" than the latter (1969, p. 168). Nonetheless, the awareness of beauty is, according to Plato and Aristotle, a necessary element in the understanding of the good, since it is this very awareness that demonstrates the inadequacy of the otherwise tempting identification of the good with the useful. The goodness for the individual of acts of justice and noble self-sacrifice cannot be explained in utilitarian terms. Yet the universal, disinterested admiration of the splendor of human virtue constitutes powerful evidence that such acts are in fact good. However inaccurate the popular or vulgar perception of virtue may be, this admiration for virtue points towards the true excellence of the philosopher; it is at the level of the goal of philosophy, wisdom, that the ultimate unity of the good, the true, the beautiful, and the useful may be recognized.[19] The excellence of the philosopher's life, in turn, serves as a justification for the praise of virtue (albeit vulgar virtue) by the political community, and its consequent refusal to identify the good with the useful.[20]

Montaigne's attack on the identification of goodness with beauty is related to his attack on the essentially dialectical character of classical philosophy: its attempt to ascend to the truth by beginning from the opinions of people whose minds are "imbued with habit," rather than from what he holds to be more certain premises derived directly from sense perception (I, xxiii, 109; II, xii, 520–22, 572).[21] He denies the reliability of judgments of beauty for a reason opposite to that cited by the ancients: not that they are too much influenced by the body, but that (by comparison with purely physical sensations) they suffer from the influence of our unstable imagination (cf. I, xxi, "Of the Power of the Imagination"). The classical philosophers' belief in an ultimate unity of the good and the beautiful, and hence of an ultimate connection between the moral opinions of the vulgar and the actual truth, is a reflection, according to Montaigne, of the essentially "poetic" character of the ancients' teaching, their portraying the natural human condition as more beautiful and elevated than it really is:[22]

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[18] III, xii, 1035. The editors of the Pléiade edition take this word to be *kaloskagathos,* but perhaps Montaigne has *kalos* alone in mind. The Pléiade editors call attention to the discussion of *kaloskagathos* in Xenophon's *Oeconomicus* (VI), a translation of which Montaigne had published under the name of his late friend La Boetie (Montaigne, 1962, p. 1667 [n. 8 to p. 1035]).

[19] Cf. Socrates' account of the "lessons" given him by Diotima: Plato, *Banquet,* 201 ff.; also, *Hippias Major,* 297b; Rosen (1969, p. 168).

[20] Cf. Aristotle, *Politics,* VII. 1–3, where the philosopher's way of life serves as the basis for the refutation of the opinion upon which existing cities are said to act (as distinguished from the moral opinions their leaders profess to the citizenry), according to which virtue is valuable only as a means rather than an end. The superiority of the philosopher's life to that of the tyrant similarly serves as the basis of Socrates' refutation of the praise of injustice in *Republic* IX. See also Rosen (1969, pp. 167–73); Strauss (1958, pp. 295–96).

[21] Montaigne's remark about "minds imbued with habit" is a quotation from Cicero's *On the Nature of the Gods,* I, 30. For an elaboration of the underlying

> And indeed philosophy is but sophisticated poetry. Whence do these ancient authors derive all their authority, but from the poets? And the first ones were poets themselves, and treat of [philosophy] in their way. Plato is but a disconnected poet. Timon calls him, as an insult, a great miracle maker (II, xii, 518).

Plato "is utterly poetic, and the old theology is poetry, the scholars say, and the first philosophy."[23] The foundations of all the various philosophic sects, Montaigne asserts (following Seneca's *88th Epistle*), were laid by Homer (II, xii, 489). In fact, Homer's greatness was such that his influence has guided "all those who have since taken it upon themselves to establish governments, to conduct wars, and to write about either religion or philosophy, of whatever sect they might be, or about the arts" (II, xxxvi, 730). Yet Montaigne casts doubt on the adequacy of Homer as a guide. Although he claims that Homer "made the infancy of poetry and of several other sciences mature, perfect, and accomplished," despite the fact that "things at birth are ordinarily imperfect," he does not indicate that the Greek poet's competence extended to *all* the sciences the foundations of which he laid. That competence must be doubted, since Homer lived "before the sciences were reduced to rules and certain observations" (II, xxxvi, 730–31). Perhaps the primitiveness of the sciences of his time prevented Homer from adequately grasping or conveying the true rules of *political* science. Indeed, Homer may not have been aware of, or may have doubted, the very possibility of a science of politics.

That political life does not rest on adequately rational or scientific foundations Montaigne indicates by remarking the unreasonableness and the injustice of the laws and customs by which people are bound. Anyone who emulates Montaigne by questioning established beliefs "will find many things accepted with undoubting resolution, which have no support but in the hoary beard and the wrinkles of the usage that goes with them" (I, xxiii, 116). "Our very laws," one will discover, provide "an apprenticeship and excuse for all sorts of wicked enterprises."[24] What people call justice is merely "the hodgepodge of the first laws that fall into our hands, and their application and practice, often very inept and very iniquitous" (II, xxxvii, 744).

The root of the deficiencies of political life, Montaigne suggests, is the popular conjunction—mirrored in the teachings of the philosophers—between the good and the beautiful, and the corresponding disjunction between the good and the useful. In "Of the Useful and the Honorable" [*honnête*], he repeats his denunciation of the injustice of existing laws, by which many vicious actions are not only permitted but also promoted. Immediately thereafter he refers to the "common language, which distinguishes between things useful and honorable, so that it calls dishonorable and foul some natural actions that are not only useful but necessary" (III, i, 773–74). But "common opinion," he asserts elsewhere, "is false" (II, xi, 401; cf. III, xiii, 1090: "The people are wrong"). Contrary to the common thinking on the particular point at issue, Montaigne elsewhere cites without criticism the "ancient notion" that "all that is useful is also just and honorable," criticizes the view that actions are less honorable if they are necessary, and asserts that pleasures which are

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reasoning and the implications of Montaigne's refounding of science on the basis of sense perception as distinguished from opinion, see Schaefer (1979).

[22] Cf. the mockery of the philosophers' "presumption" at II, xii, 468–69; also the stress on humanity's natural ugliness at II, xii, 463–64.

[23] III, ix, 794. The Pléiade edition traces this assertion to St. Augustine's account of Varro's opinions in *The City of God* (Montaigne, 1962, p. 1655 [n. 1 to p. 974]).

[24] The central intention of the chapter "Of Custom, and Not Easily Changing an Accepted Law," from which these remarks are taken, is to challenge Aristotle's counsel against making frequent changes in the law (*Politics*, II. 8). The premise of Aristotle's argument is that a people's obedience to law is dependent on habit rather than on reason; from this he infers that politics cannot be a science like medicine. Montaigne's critique of Aristotle's argument indicates his belief that politics can in fact be made into a science, or in other words, that political life can be rationalized.

natural "are therefore necessary *and just*" (III, xiii, 1048, 1088, 1094, emphasis added). Montaigne's argument and examples in III, i, demonstrate the way in which the disjunction between the useful and the honorable promotes injustice; tyrannical rulers, "impostors," and "worthless men," acting for the sake of what is useful to themselves, cover their actions with the *pretense* of virtue or exercise a hypocritical remorse to deceive the people.[25] Their pretense of giving up "the useful for the honorable" (III, i, 767) deceives the people into the loss of what is useful to themselves for the gain of their deceivers.

At the conclusion of III, i, Montaigne urges his readers to "take away from wicked, bloody, and treacherous natures" the pretext of reason that demands the entire subordination of the private interest to the alleged public good, and that in fact results in the sacrifices of other people's interests to those of the wicked. He then remarks that "we poorly argue the honor and beauty of an action from its utility, and we commit a fallacy in thinking that everyone is obliged to perform—and that it is honorable for everyone to perform—an action merely because it is useful." Here, the intention of the disjunction between utility on the one hand and honor and beauty on the other seems to be to urge people to pursue the latter qualities without regard to the former. But the essayist's supporting example—the Church's requirement of priestly celibacy, on the ground that it is more honorable than marriage, despite the fact that the latter is "the most necessary and useful action of human society"—makes his praise of honor over utility appear ironic (III, i, 781). The correctness of this suspicion is demonstrated elsewhere by the essayist's condemnation of those who foolishly scorn the sexual act "as shameless and indecent, blush at it, and recommend abstinence" (III, v, 856).

Montaigne's own intention consists in the severance of the connection between the good and the beautiful, to conjoin the former with the useful instead; and the redefinition of honor and virtue to conform to this new understanding of goodness.[26] The ground and nature of this intention are further elaborated in a chapter entitled "Of Honorary Awards" (or "Of the Rewards of Honor"). Here the essayist notes the disposition of "men of quality" to pursue "vain and valueless marks of honor" such as the orders of knighthood as a reward for their virtue, in preference to rewards in which there is "gain and profit." Montaigne praises the practice of issuing such awards, since they are "no load on the public and cost the prince nothing" (II, vii, 360–61). At the same time he tacitly questions the understanding of virtue on which these awards are based. The etymology of the word reveals the way in which it is understood:

> It is worth considering . . . that our nation gives valor [*vaillance*] the highest rank among the virtues, as its name shows, which comes from value [*valeur*]; and that when we say "a very valuable man," or "a worthy man," in the language of our court and our nobility, we are saying nothing else than "a valiant man," as in the Roman fashion; for the Romans took the general term "virtue" from their word for strength. The proper, the only, the essential form of nobility in France is the military profession (II, vii, 363).

French morality, which honors courage as the essence of virtue, is the descendant of Roman morality.[27] Yet, even leaving aside the issue of whether virtue is reducible to valor, this morality rests on a defective understanding of valor itself. The valor that the French and the Romans honor is far inferior to true, "philosophic" courage, "a valor that is a strength and assurance of the soul, equally despising all sorts of adverse accidents, equable, uniform, and constant, a valor of which ours is only a very feeble ray." The inferiority of popular courage is demonstrated by its very conventionality; it is established by "habit, education, example, and custom," which "easily make it common, as is easy to see by the experience of it which our civil wars afford us" (II, vii, 362).

Popular, vulgar courage aspires to honor rather than utility as its goal. Yet Montaigne traces the original identification of virtue with vulgar courage to its utility:

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[25] III, i, 767, 774–79, and passim, esp. 776. Cf. also Machiavelli's reference to the "holy cruelty" practiced by Ferdinand of Spain (1964, Ch. 21, p. 185); Strauss (1958, pp. 186–87).

[26] Cf. the counsel at III, xii, 1016 to resist the temptation to "call . . . good what is only beautiful." Although Montaigne asserts at III, ii, 784 that true vices are necessarily ugly to people of sound judgment, he limits the bearing of this rule a few pages later by observing that the ugliness of vice is no longer apparent to those who have become attached to it by nature or habit, and tacitly rejects the Socratic dictum that no man does wrong knowingly (III, ii, 789–90). Here as elsewhere, there are important connections between Montaigne's project and that of Descartes: cf. Descartes' *The Passions of the Soul,* lxxxv; also, Rosen (1969, pp. 65–66n.).

[27] Cf. Plutarch's comment on the Roman equation of virtue with valor in his "Life of Coriolanus" (Plutarch, p. 263).

> It is probable that the first virtue that manifested itself among men and gave some the advantage over the others was this one, by which the strongest and most courageous made themselves masters of the weaker, and acquired particular rank and reputation, whence it has retained this linguistic honor and dignity; or else that these nations, being very warlike, gave the prize and the worthiest title to the one virtue that was most familiar to them (II, vii, 363–64).

Montaigne's account of the origin of the popular notion of virtue suggests both its primitive character and its injustice. Vulgar courage was identified with virtue by ignorant, warlike people who as yet saw no possibility of a higher kind of human excellence, that of the mind: the most courageous people, in the vulgar sense, are the cannibals.[28] At the same time vulgar courage is one of the means by which the strong enslave the weak, thus giving rise to the injustice which we have seen Montaigne lament.

One way in which Montaigne attacks the popular understanding of virtue as valor is to suggest its inutility. He questions, in particular, the conduciveness of the heroic deeds and high standards of Cato the Younger to the welfare of Rome. In "Of Cruelty," he suggests, even while professing not to believe, that Cato was really uninterested in his country's well-being, as compared with his own glory: Cato might have been "grateful to fortune for having put his virtue to so beautiful [*belle*] a test" and for enabling Caesar to destroy "the ancient liberty of his country" (II, xi, 403). Regardless of Cato's real motivation, the essayist elsewhere doubts the utility of the *effects* of his conduct for his country. He observes that although the younger Cato's virtue was "much more spotless" than that of Cato the Censor, the latter "excelled the other in military exploits and in the usefulness of his public services." This remark occurs in a chapter entitled "All Things Have Their Season"; Montaigne elaborates this title in the text with the remark, "good ones and all" (II, xxviii, 680–81). In other words, what are popularly regarded as bad things are sometimes necessary ones (as noted also in III, i); and the most spotless virtue is not always most beneficial in its effects (cf. Machiavelli, *The Prince,* Ch. 15). Montaigne makes the same point about Cato the Younger more explicitly in a later chapter:

> Life in society should have some relation to other lives. The virtue of Cato was vigorous beyond the measure of his time; and for a man who took a hand in governing others, a man dedicated to the public service, it might be said that his was a justice, if not unjust, at least vain and out of season (II, xi, 969).

Montaigne dwells further on the limitations of the popular morality in the fifth and sixth chapters of Book I, where the use of valor is compared with that of cunning as a means to victory. He first seems to endorse the scrupulousness of "the old men of the [Roman] Senate," who condemned the practice of trickery against an enemy "as hostile to their old way: which was, they said, to fight by virtue, not by craft" (I, v, 27). Similarly, in the following chapter, Montaigne appears to admire Alexander's "generous" refusal to "steal" a victory (I, vi, 31). But neither of these remarks suggests that to rely solely on one's own valor is the best means to victory. To the contrary, Montaigne demonstrates the success that may be achieved through trickery. Moreover, he indicates that, given the general unscrupulousness of human beings, it would be extremely imprudent to adhere to the strict principles professed by the senators. After contrasting the old Roman ways with "those of Greek subtlety and Punic cunning," he observes that even "in the age of the justest captains and the most perfect Roman discipline," the Romans took advantage of their enemies' "stupidity" (I, vi, 30). The old men of the Senate exemplify the disposition of old age to "praise the past and blame the present" (II, xiii, 589), evidently forgetting what the past was really like; like other conservatives, they try to make the beginnings of their regime look better than they were. In opposition to their policy, Montaigne observes that "war has by nature many privileges that are reasonable even at the expense of reason." Although he professes to be "astonished at the range Xenophon gives these privileges," given his "marvelous weight," the essayist does not dissent from the Greek's counsel beyond remarking that he does "not agree to the extent of his dispensation in all things and throughout" (I, vi, 30–31). He does not inform us of any points on which he disagrees with Xenophon. In fact he expressly distinguishes between those people, some of them "barbarians," who scorn to use deceit, and "us, who, less superstitious, hold that the man who has the profit of war has the honor of it" (I, v, 28). There is no reason to assume that Montaigne's use of the first person plural is ironic.

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[28] I, xxxi, 207. Vulgar courage and philosophical (Socratic) courage are also tacitly contrasted in III, vi: see Trafton (1973).

The critical premise of those who scorn victories won by craft is that only contests of valor test a person's real worth: "only that man considers himself overcome who knows he was downed neither by trick nor by luck but by valiance, man to man, in a fair and just war" (I, v, 27). But, partly through the use of quotations from Cicero and Ariosto (v, 28; vi, 31), Montaigne questions the antimony this premise embodies between valor and fortune. Fortune, not valor, determines the outcome of battles between those who scorn to employ craft, and the achievement of the glory that virtuous human beings seek (I, xxxi, 210; II, xvi, 605). The true essence and quality of a person's self is to be found in the mind, rather than in actions or bodily strength (II, vi, 359; II, vii, 380). The identification of virtue with valor is the heritage, as we have seen, of a primitive mentality that was simply incapable of imagining a truer form of human excellence. The consequence of this identification is not only injustice but also cruelty: "generous" in scorning to win by trickery, Alexander was at the same time guilty of "the ruin of Thebes, the murder of Menander, of Hephaestion's doctor, of so many Persian prisoners of war at one stroke, of a troop of Indian soldiers not without prejudice to his word, and of the Cosseians even to their little children."[29] The most "honest" race of warriors portrayed in the *Essays* are the cannibals, the "barbarous horror" of whose actions Montaigne readily acknowledges—even while lamenting the blindness of civilized people to the barbarity of some of their own deeds. While the cannibals have no words in their language for lying, treachery, or dissimulation, and fight by valor not trickery, they also lack a word for pardon, and their battles "never end but in slaughter and bloodshed." (Montaigne again indicates the ambiguity of "beauty" and its unreliability as a standard of virtue by calling the cannibals' warfare "as . . . beautiful as this human disease can be" [I, xxxi, 204, 207–08, 210].)

To demonstrate the inutility and the illogic of the morality of valor, however, does not suffice to undermine it. Even though this morality originated as the praise of a quality that was thought to be useful, it derives its deeper strength, as we have seen, from another source: the identification of the good with the beautiful, and the belief that heroic virtue, of the kind exemplified by Cato, is beautiful because it exemplifies human self-transcendence. If Montaigne is to succeed in "obliterating and abolishing" the present morality and establishing a "new order" to replace it,[30] he will not only have to demonstrate the possibility of a more advantageous order, but will also have to undermine the moral-aesthetic appeal of the present one. To accomplish the latter goal is a major function of the author's rhetoric throughout the *Essays*.

Montaigne represents himself and Cato as the embodiments of two radically divergent conceptions of virtue, and tacitly invites the reader to choose between them. Cato, on the one hand, embodies the old morality of self-struggle, of the striving for a godlike excellence and glory. Montaigne, on the other hand, would teach his readers a "new lesson: that the value and height of true virtue lies in the ease, utility, and pleasure of its practice, which is so far from being difficult that children can master it as well as men, the simple as well as the subtle."[31] The comparison between the essayist and Cato is hardly presented in such a way as to facilitate an objective choice on the part of the reader. In the passages (most frequent in the early chapters of Book I) where he develops the classical, Stoical conception of virtue exemplified by Cato, Montaigne largely parrots the sayings of ancient authorities; the unoriginal character of these passages has misled the critics into thinking that they represent an early phase of Montaigne's "development" (see Villey, 1908, Vol. II, pp. 38–70; Zeitlin, 1935, Vol. I, pp. lii–lvi), when their appearance is in fact an instrument of his art. Montaigne wants to stress the ugliness rather than the beauty of Stoicism and asceticism: having asserted it to be "the duty of good men to portray virtue as being as beautiful as possible" (I, xxxvii, 227), he chooses to present Cato as the supreme "example of virtue . . . in his proudest posture . . . all bloody, tearing out his own bowels, rather than sword in hand, as did the statuaries of his time" (II, xiii, 594–95). In the light of this portrayal, we are not surprised by Montaigne's judgment that "so beautiful an action would have been unbecomingly located in any other life than Cato's" (II, xi, 404). Montaigne, on the other hand, presents his own life as a model that *is* to be imitated by the generality of people (see II, xviii, 646). His way of life as he commonly presents it is an unambitious one, dedicated to the enjoyment of the present pleasures of earthly life, and to the pursuit of no glory other than that of tranquility (II, xvi, 605).

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[29] At I, i, 13, Montaigne claims that Alexander was "very gracious to the vanquished," but his evidence contradicts this claim. If Alexander's anger could ever "have been bridled, it is probable that it would have been in the capture and desolation of the city of Thebes. . . . Yet the distress of [the Thebans'] valor found no pity, and the length of a day was not enough to satiate Alexander's revenge" (p. 14).

[30] II, vii, 363. Although the "new order" to which Montaigne seems at first sight to be referring in this context is merely a replacement for the Order of St. Michel, a French order of nobility, his reference to the "old order" that is to be replaced is indefinite ("this one"), and follows a discussion of the difference between the vulgar kind of courage that the Order of St. Michel rewarded and true, philosophical courage. Moreover, the reader of the *Essays* must always bear in mind Montaigne's remark that his stories "often bear, outside of my discourse, a subtler note, both for us, who do not wish to express anything more, and for those who get my drift" (I, xl, 245). (Cf. also Machiavelli's discussion of "new modes and orders" in chapter 6 of *The Prince*.)

[31] I, xxvi, 161. This remark contrasts strikingly with Aristotle's moral teaching (cf. *Nicomachean Ethics* I. 9, 1100a 1–5), and makes evident the egalitarian character of Montaigne's morality. Cf. I, xx, 92: "Equality is the principal part of equity." (One can trace in Montaigne's new morality the root of Rousseau's doctrine of "natural" goodness, with its similarly egalitarian connotation.)

There is an integral relation between the way of life Montaigne advocates and and his rhetorical mode of making it appealing. The fundamental rhetorical feature of the *Essays* is the author's profession of frankness: of lowering his guard, not pretending to virtues he does not possess, admitting his vices and defects. But as Rousseau, perhaps a more genuinely honest autobiographer, observed, the only vices to which Montaigne admits–such defects as laziness, forgetfulness, self-indulgence, and an unwillingness to abide restraints–are "lovable" ones, which serve to enhance the charm of the author's self-portrait (Rousseau, 1959–69, Vol. I, p. 1150; cf. Zeitlin, 1935, Vol. II, pp. 564–70). At the same time, Montaigne suggests that these putative vices are inextricably linked with his distinctive virtues: tolerance, honesty, compassion, an abhorrence of cruelty, and a disinclination to meddle with other people's affairs (cf. II, xvii, 629). Precisely that lack of ambition for glory and greatness that allegedly makes him lazy and allows him to own up to his defects in public also saves Montaigne from wanting to oppress others. Hence, while unthinking critics might lambaste his administration as mayor of Bordeaux as a do-nothing one, he in fact omitted only those actions "that ambition mixes up with duty and covers with its name," not having "that iniquitous and rather common disposition of wanting the trouble and sickness of the affairs of this city to exalt and honor my government" (III, x, 999, 1002).

In sum, that innocence which Montaigne professes in "Of Cruelty" and elsewhere, despite his seeming denigration of it, is in fact (in his opinion) far more beneficent in its effects than Cato's virtue, whatever the beauty of the latter. The point of Montaigne's self-deprecatory comparison of himself to Cato in "Of Cruelty" has been well grasped by Jacob Zeitlin (1935), who aptly describes the essayist's "insinuating method" (Vol. II, p. 475):

> Being revolted at the brutality and callousness which were common features in the life of his age, and desiring to oppose to them the finer feelings of his own nature, could he possibly effect his object by parading his sensibility as something that set him above his fellows? Not only would it be contrary to the manners of a French gentleman; it might even fail to get a sympathetic hearing in a society which regarded the courage of the soldier as the greatest virtue . . . and the hunt as the noblest manly activity. But if you somewhat guiltily refer to the trait as a mark of your softness and weakness, then your reader is apt to follow you with complacent indulgence. . . . Such sympathy for the sufferings of helpless animals [as Montaigne expresses] is well calculated, Montaigne knows, to make his contemporaries laugh at him. . . . But while they are laughing it is easy to step inside their guard and deliver a few piercing thrusts, to accuse the age of bloodthirstiness and cruelty, to grow indignant at the "incredible examples" of barbarism and inhumanity in which the time abounds, and to censure the established system of judicial torture. . . . To advance such views with positive and solemn formality would savour a little too much of the preacher and it might also expose one to annoying inquisitions from the guardians of religion . . . but, in the midst of a farrago of such eccentric sentimentalities, a few critical judgments can be slipped in without greater danger. They could not be taken with sufficient seriousness to arouse official prosecution, yet with their oblique movement they might succeed in reaching the more accessible and generous spirits (Vol. II, pp. 476–77).

Lucid as Zeitlin's analysis is, it stands in need of correction in one respect, and elaboration in another. Because he takes for granted the evolutionary interpretation of the *Essays,* and takes too literally Montaigne's self-disparagement, Zeitlin presumes that at the time of the first edition of the *Essays* Montaigne "has not yet arrived at the idea that the goodness which we owe to our nature is perhaps of a higher kind than that which man attains by his own effort" (Vol. II, p. 475), and that the essayist's self-deprecation is a remnant of his earlier attitude. But does not Zeitlin's own analysis justify the conclusion that Montaigne's claim of inferiority to Cato is ironic, and that he fully intends the reader to recognize the superiority of his "goodness," at least in its *effects,* to the excellence he attributes to Cato?

What needs to be added to Zeitlin's analysis is a consideration of Montaigne's account of the resemblance of human beings to the beasts, and the critical role of this argument in justifying humaneness or mildness towards the human species. This argument demonstrates Montaigne's awareness that mere admonition, of the kind Zeitlin describes in "Of Cruelty," could hardly suffice to mitigate human cruelty, especially given the naturalness he attributes to the "instinct for inhumanity." For Montaigne the brutality and callousness of his age are symptomatic of a defective morality that characterizes *all* heretofore existing civil societies. The overcoming of cruelty thus requires an entire transformation of humanity's self-understanding and a rechanneling of its desires, of a kind that is suggested by the movement in "Of Cruelty" from the divine to the animal.

The practical import of this movement is indicated by another chapter, "Of Drunkenness," which moves in the opposite direction: from the "brutish" to the striving for godlike virtue. Montaigne opens by attesting to the need that "we lesser men" have for a "science of distinguishing among the vices," as a substitute for the wisdom Socrates sought in order "to distinguish good things from bad." He proceeds to denounce drunkenness as a "bodily and earthly" and hence "brutish" vice, in contrast with other vices that seem "noble" [*généreux*]. Yet after this denunciation the status of drunkenness rises rapidly, culminating in the essayist's recommendation that "we should refuse no chance to drink, and have this desire always in our head" (II, ii, 332, 325).

Montaigne indirectly suggests the reason for this shift in his position. At first he asserts that drinking interferes with another supposed vice, lechery (II, ii, 325). Elsewhere, however, he denies this claim, noting that "Venus and Bacchus are prone to go together" (II, xxxiii, 709). But the essayist proceeds in "Of Drunkenness" to introduce another form of what metaphorically may be called "drunkenness": the "madness" of people who profess an extreme hatred of sensual pleasure, or who are carried away by "the heat of combat" (II, ii, 329–30). This form of intoxication constitutes the truly dangerous sort of vice that Montaigne believes would be checked by a greater acceptance of physical pleasure, such as imbibing in the literal sense. The self-indulgence that is exemplified in drinking, however "gross and brutish" it may seem, is "less malicious and harmful" than other vices, "which almost all clash more directly with public society."[32] Hence it is far preferable to the beautiful but terrible "virtue" of a Cato, or of those who seek to "gratify heaven and nature by committing massacre and homicide, a belief universally embraced in all religions" (I, xxx, 199). Caesar and Alexander, who so troubled the world in their quest after glory and greatness, would have been far wiser to regard the enjoyment of "natural and therefore necessary and just pleasures"—the pleasures of the body—as their proper occupation.[33]

Montaigne's practical moral teaching amounts to an advocacy of de-sublimation.[34] He describes nature as the source of humanity's commonest error, that of "always gaping after future things" in place of "present goods" (I, iii, 18). (As the "Apology for Raymond Sebond" makes evident, the "future" goods referred to include the religious hope of an afterlife, as well as the pagan hero's goal of earthly glory.) At the same time, the essayist remarks (in the title of I, iv) that "the soul discharges its passions on false objects when the true are wanting." He implies that people's tendency to waste their time in the pursuit of "imaginary and fanciful goods, goods future and absent" (II, xii, 464) rather than real and earthly ones could be overcome if they were taught the choiceworthiness of the latter, if the moral and religious restraints on self-indulgence were eliminated,[35] and if society were dedicated to making the physical conditions of existence more comfortable.[36] The process of redirecting people from the serious contemplation of their mortal condition, and of what that implies regarding the purpose of life, to the enjoyment of corporeal pleasures, is aptly described by the title of III, iv: "Of Diversion."[37]

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[32] II, ii, 324. Montaigne's Rabelaisian rhetoric on behalf of immoderate drinking invites comparison to the Athenian stranger's recommendation of *regulated* drinking parties in Plato's *Laws* (639 ff.), to which the essayist alludes in this chapter (II, ii, 327). Compare also Montaigne's reference to the ancient philosophers' moderation with respect to sensual pleasures (II, xii, 569) with I, xxx, where indulgence in such pleasures is defended as an alternative to "immoderation . . . in the direction of the good" (195, 198).

[33] III, xi, 1088. In the earlier, 1588 edition these pleasures are explicitly said to be "corporeal" (Montaigne, 1962, p. 1676 [n. 1 to p. 1088]).

[34] Cf. III, v, 855, where Montaigne describes Venus as "nothing else but the pleasure of discharging our vessels," in contrast to Socrates' view that "love is the appetite for generation by the mediation of beauty."

[35] See especially II, xii, 566–69; III, v; and Montaigne's account of his way of life in III, xiii, 1057–94. Cf. also I, xiv, 65, regarding the difficulty of setting limits to people's desire for things they consider good, and the need to limit the impulse towards frugality; this passage recalls Machiavelli's

Recognizing, as the ancient philosophers did, that the human being's bipartite character requires us to understand ourselves in the light either of the divine or of the animal (cf. Strauss, 1958, p. 78), Montaigne chooses to point humanity in the opposite direction from that advocated by the classical thinkers. Partly in order to promote the well-being of the community as a whole, the ancients tried to "distract" us from ourselves, "assuming that we were attached to ourselves only too much and by too natural a bond" (III, x, 983; cf. III, ix, 967–68). Montaigne, on the other hand, preaches selfishness, and indulgence in the bodily pleasures that privatize human beings. He would divert humanity, not from itself, but from the soul's natural desire to look beyond itself.

Montaigne's severance of the good and the beautiful entails the severance of our judgment of ourselves from our feeling about ourselves.[38] In order to combat the human pride that gives rise to Catonic virtue, Montaigne embraces those opinions "that despise, humiliate, and nullify" the human species (II, xvii, 617). Hence he dedicates the longest chapter of the *Essays*, the "Apology for Raymond Sebond," to demonstrating the foolishness and unjustifiability of human pride. But unlike Christian writers who adopted similar themes, his goal is not to teach people the need to humble themselves before God and do penance for their sins, but to accept their nature as it is, without aspiring to an elevated condition that is beyond their reach.[39] In order to prevent the awareness of people's defects from continuing to stimulate the religious quest for self-improvement through sacrifice and penance, Montaigne tries to set an example of how one can combine a smallness of self-esteem with a greatness of self-love (II, xvii, 641). What is required is a self-love that is unjustified except by the fact that such self-love is natural; any attempt to justify one's self-love objectively would cause the individual to condemn self-indulgence, since it provides no ground for self-esteem.[40] The thrust of Montaigne's argument points towards the replacement of Christian charity by modern "humanism," which ordains that people should be treated decently, not because they were made in the image of God, but merely because (as suggested by the conclusion of "Of Cruelty") they are sentient beings with whom one can sympathize. Humanistic compassion, unlike the religious motive, need not entail regulating human life so as to improve people's souls: the proper goal of humanism is limited to the negative one of relieving humanity from pain.[41] It is precisely because Christian theology aspires to elevate humanity that Montaigne finds it inferior even to the pagan doctrines of metempsychosis that link humanity to the beasts: "supercelestial opinions" are commonly accompanied by "subterranean morals" (III, xiii, 1095).

Montaigne's endeavor to transform people's self-understanding in order to make their treatment of each other more humane is motivated in part by his own "natural compassion, which has infinite power over me," and which leads the essayist to sympathize in particular with "that class of men that needs our help," the common people (III, xiii, 1079). He is more prone to be influenced in other people's favor by pity for their sufferings than likely to yield to esteem for their excellence (I, i, 12). But despite his profession of softness, it would be wrong to be misled by Montaigne's pretended frankness into identifying the author with the

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remark concerning the naturalness and unlimitedness of the desire to acquire things (Machiavelli, 1964, Ch. 3, p. 23).

[36] Montaigne's project for the transformation of science into "medicine," broadly understood, is discussed in Schaefer (1979).

[37] On the problems this entails, cf. the discussion of divertissement in Pascal's Pensées.

[38] Cf. III. xiii, 1052: "Judgment holds in me a magisterial seat, at least it carefully tries to. It lets my feelings go their own way, both hatred and friendship, even the friendship I bear myself, without being changed and corrupted by them. If it cannot reform the other parts according to itself, at least it does not let itself be deformed to match them; it plays its game apart."

[39] Cf. III, ii: man's nature is unreformable (782); attempts to extirpate his essential qualities only "cover them up" (788). It is foolish to repent that one's nature is neither that of "an angel nor Cato," since such things "are not in our power" (791).

[40] Cf. Aristotle, *Nicomachean Ethics*, IX. 8, on the difference between true and vulgar self-love.

[41] Cf. II, xii, 472: "The misery of our condition makes us have less to enjoy than to avoid, and makes the extremest pleasure not touch us so much as a slight pain. . . . Our well-being is but the privation of being ill."

unimposing, albeit appealing, self-portrait he provides in "Of Cruelty" and elsewhere. He admits that his service to the many may be the result of a desire for glory as well as the sentiment of compassion (III, xiii, 1079). He hints at the hope of achieving through his book a glory surpassing that of the greatest lawgivers, by acting as a *teacher* of legislators (II, viii, 380–83; III, xiii, 1055). Yet throughout the *Essays* Montaigne has sought to deter people from the pursuit of glory by stressing its irrationality. It would seem that Montaigne's "new order" can succeed only by leaving people like Montaigne out of account.[42] In order for philosophy to be useful to society in general, both its beauty and the motives that underlie it must be obscured or denied.[43] The contemporary decay of political philosophy into ideology—indeed, the near-effacement of the distinction between the two—may cause the present-day reader to wonder about the ultimate adequacy of Montaigne's project. Another aspect of the difficulty is reflected in Max Weber's observation that "since Nietzsche, we realize that something can be beautiful, not only in spite of the aspect in which it is not good, but rather in that very aspect" (Weber, 1958; p. 148). Nietzsche's denunciation of the "herd morality" that equates the good with the useful constitutes a most serious challenge to the understanding of humanity on which the modern liberal regime seems to rest.[44] Yet the contemporary critic of bourgeois materialism ought not to forget the connectedness of this kind of materialism with the virtues of moderation and tolerance, as Montaigne articulated it; and the dangers that he showed to be connected with an overestimation of humanity's status in the universe.

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[42] Cf. the analysis by Harvey C. Mansfield, Jr. (1971, p. 50) of "the self-forgetting of the modern idealist" as exemplified in the thought of Thomas Jefferson. On the problems associated with the connection between science and beneficence in modern thought, cf. Jonas (1966, pp. 193–97).

[43] Cf. Montaigne's summary of Socrates' defense from Plato's *Apology of Socrates* at III, xii, 1030. By transforming Socrates into a materialist ("not being, as Homer says, born of wood or stone, *any more than others*"), and by identifying the Athenian with the belief he attributes to the many, including the Persian king, that a dreamless sleep is the greatest of life's pleasures, Montaigne cuts off Socrates' view of a positive purpose to life to be achieved through philosophy. At the same time, he has Socrates profess to teach not only "just" but *"useful"* things to the many, thus defending philosophy on a new ground (emphasis added in both quotations; compare *Apology of Socrates* 31a–32a, 34d, 38a, 40d).

[44] This essay has been intended in part to challenge Nietzsche's attribution of the equation of goodness with utility (in the sense of the mere satisfaction of corporeal needs) to Socrates. (See Friedrich Nietzsche, *Beyond Good and Evil,* Sec. 190.) At least as Montaigne presents the issue, that attribution is incorrect: the true founder of the "moral utilitarianism" denounced by Nietzsche is not Socrates, but Montaigne and his philosophic successors (such as Hobbes and Locke). To challenge modern "rationalism," therefore, need not entail rejecting philosophy, or subordinating the quest for truth to the "will to power." On the connection between the reduction of the good to the useful in early modern philosophy and the roots of contemporary nihilism, see Rosen (1969, pp. 65 ff.).

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Metaphor and Political Knowledge*

EUGENE F. MILLER
University of Georgia

Since the language of political inquiry seems to be inescapably metaphorical, the question necessarily arises as to how metaphors of various types, including models, enter into the composition and expression of political knowledge. The solutions that have been most influential in contemporary political science can be called the verificationist and constitutivist views of political metaphor. While both views contain important elements of truth, there are fundamental difficulties in each that require the search for a more satisfactory view. An alternative view of metaphor and political knowledge is developed by reference to four main problems: Why is political speech metaphorical? How do metaphors make political things manifest? How are political metaphors tested? and Are metaphors indispensable to political expression and political knowledge?

Political inquiry has provided fertile soil for the growth of metaphors. For some who have cultivated this soil, metaphors are brambles to be cleared away, not plants that bear fruit in political knowledge. Yet even the critics of metaphors have not been able to avoid them. In the *Leviathan,* for example, Hobbes makes a strong case against the use of metaphors in science:

> The light of human minds is perspicuous words, but by exact definitions first snuffed and purged from ambiguity; *reason* is the *pace;* increase of *science,* the *way;* and the benefit of mankind, the *end.* And, on the contrary, metaphors, and senseless and ambiguous words, are like *ignes fatui;* and reasoning upon them is wandering amongst innumerable absurdities; and their end, contention and sedition, or contempt (1955, pp. 29–30).

Despite these harsh words, Hobbes does not hesitate to embody a metaphor in the title of *Leviathan.* His introduction draws an extended comparison between the commonwealth and a natural man, and metaphors abound in the work itself. Most striking of all is the fact that in the very passage where he deprecates metaphorical reasoning in contrast to science, Hobbes makes use of a fine metaphor—that of traveling a path. Metaphors themselves are said to be like "foolish fires."[1]

Whether intended or not, the contradiction between Hobbes' principle and his practice teaches an important lesson about metaphors and models in political science. As Landau observes, "political science has always resorted to metaphors, to the device of proceeding from the known to the unknown. Those who criticize the use of models need to understand that they too must use them" (1972, pp. 101–02). Contemporary theories that follow the methods of modern science are as dependent on metaphor as the classical theories of the great political philosophers. This dependence shows that political inquiry of a scientific or philosophical character cannot break completely with the pattern of ordinary speech, which is also highly metaphorical. People in political life speak of the "players" on the president's "team," of "cutting fat" from a budget, of the "cold war," "brainwashing," "detente," the "melting pot," the "new frontier," and the "war on poverty." Any effective orator knows what treatises on rhetoric have always pointed out—that political speeches are enlivened and made more persuasive by metaphors.

Reflection on metaphor leads into the deepest problems that can arise about political knowledge, including the problem of how we know anything political at all. Contemporary writings in political science suggest two alternative views of how metaphors enter into the composition and expression of political knowledge. These alternatives conform, more or less, to broader philosophical currents and to interpretations of metaphor that these currents have produced. For convenience, I shall call these the *verificationist* and the *constitutivist* views of political metaphor. These names are meant to suggest how each view would account for the relationship of the metaphor to the object of knowledge. The verificationist view of political metaphor grows out of modern empiricism, while the constitutivist view is rooted in counter-empiricist philosophy as it developed after Kant. Each of these views has something important to say about the nature of political metaphor, but each view is marked by its own peculiar difficulties. My article will seek to provide an account of political metaphor that can meet these difficulties. It can perhaps best be designated as a *manifestationist* view.

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[1] According to the *Oxford English Dictionary, ignis fatuus* ("foolish fire") was used in Hobbes' time to refer to the phosphorescent light seen hovering or flitting over marshy ground, which appears to recede and finally to vanish when approached. The term was used figuratively for any delusive guiding principle, hope, aim, or such.

Political Metaphor

Let us begin by considering what metaphor is and by reviewing some of the phenomena that any interpretation of political metaphor should be able to account for. "Metaphor" comes from *metapherein,* which means to carry from one place to another. "To transfer" is a good equivalent of the Greek verb. In metaphor, what is transferred is a word and at least some of the meaning that the word ordinarily conveys. The word that is used to name one thing is applied to something else. Metaphor typically involves the transfer of a name to something of a different kind, or "sort crossing," as Turbayne (1962, p. 12) has aptly called it. According to Aristotle's influential analysis, this transference rests on the likeness that is possible between different kinds of things. If the same name is given to things that are so alike as to belong to the same species, we have univocal naming. If there is no likeness at all between the thing named and the thing from which the name is taken, we have pure equivocation. Metaphorical likeness thus falls somewhere between the likeness that members of the same species bear to one another and the absence of a significant likeness. Four types of metaphor are recognized by Aristotle, and each reflects a certain likeness or proportion. These involve transference from genus to species, transference from species to genus, transference from species to species, and transference from analogy.[2]

While Aristotle uses "metaphor" broadly in the *Poetics* for various types of transference, including analogy, medieval Aristotelianism tended to make "analogy" the inclusive term and to treat metaphor as an "improper" kind of analogy or proportionality (see McInerny, 1961, 1968; Burrell, 1973). In modern discussions, the transference that takes place in analogies, similes and models is sometimes distinguished from metaphor and sometimes subsumed under it.[3] I shall be using "metaphor" inclusively for all types of transference of words and meaning from one kind of thing to another. I shall distinguish later between analogical and nonanalogical metaphors and between metaphorical and nonmetaphorical models.

The dynamics of metaphor can be seen in this rather commonplace example: "Washington is the father of his country." In this sentence, "father" is used metaphorically. The word has been transferred from one sphere of relationships to another. "Father" refers ordinarily to a family member, and its primary or literal meaning is determined by family relationships. Our sentence serves to indicate the new context in which "father" is being used. Here the word refers to a general and statesman, not to a biological father, and it specifies that person's relationship to a political community, not to a natural family. Interpreting "father" literally in this context would lead to misunderstanding and absurdity, and yet the metaphor does not work unless at least some of the word's literal meaning is carried over and applied to the new referent. Thus if we found it necessary to explain the meaning of our sentence to a literal-minded friend, we might mention that the country was born through a war of independence that Washington led, that Washington took care for his country's upbringing by helping to establish its Constitution and by serving as its first president, and that he has gained the respect and reverence of the people of his country.[4]

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[2] *Poetics* 1456b 7–1458a 8. An example of the first type of metaphor is Aristotle's own use of *politeia,* the generic name for all regimes, to designate the particular species of regime in which the middle class rules. An example of the second type would be calling all rulers "tyrants" or all symbols of authority "scepters." When a political ruler is called by a name appropriate to another species of rule, such as "father," we have the third type of metaphor. When a name such as "shepherd" is used, suggesting an analogy between political rule and a very different activity, we have the fourth type.

[3] For an interesting account of political metaphor that deems it important to distinguish both analogy and simile from metaphor, see Zashin and Chapman (1974). The authors contend that metaphor differs from simile and analogy by asserting an identity of subjects, not merely a likeness. Our awareness of the subjects' differences makes this assertion of identity anomalous and disturbing, thus giving rise to a tension which leads us to search for a basis for identity (or, using Black's terminology, to "create" similarities) that will reduce the impression of anomaly. I do not find such a sharp distinction between metaphor and analogy or simile to be necessary, for as I shall argue, metaphor is based on the discovery of likeness. A "tension theory" need not deny this point, for as Ricoeur contends, the factor of likeness or resemblance is of even greater necessity in a tension theory than in other theories of metaphor (1977, pp. 193–95). Even so, tension theories are rendered questionable, I believe, by the fact that our first apprehension of metaphors is usually not accompanied by a sense of tension.

A great deal of attention has been given to some dominant metaphors or models that have successively influenced modern political theory—the machine, the organism, and the servomechanism. These metaphors have no doubt been important, but there is a danger of overstating the extent to which a few archetypal metaphors may have determined political thinking and of understating the great variety of sources from which political metaphors have been drawn. While no exhaustive catalogue can be attempted here, the following would have to be included in a listing of the major sources of the metaphors of political theory: *the human relationships of everyday life,* such as those in the family (father to children, husband to wife, master to slave, brotherhood), those in the sphere of exchanges, contracts, and promises, and those in sporting activities and games or in warfare and military life; *making and doing things through the arts,* such as medicine, gymnastics, generalship, horsemanship, tending sheep, piloting, building, tailoring, painting, acting, flute-playing, and engineering on a large scale; *the characteristics of artifacts* such as a building and its foundations, a pyramid, a wheel, a pair of scales, a web or cloth woven from thread, clocks, machines, pumps, engines, computers, and communications networks; *capacities of human beings,* such as in references that treat a community or institution as a "person" that plans or exercises foresight and experiences such human emotions as love, hatred, pride, sympathy, and magnanimity; *subhuman activities or processes in nature,* such as animal behavior (Machiavelli's fox and lion, Nietzsche's lambs and birds of prey), organic processes common to human and nonhuman beings (conception, birth, maturation, health and disease, death), and the natural properties or motions of bodies (revolution, attraction and repulsion, the exertion of force); and, finally, *mathematical relations* and proportions of various kinds.[5]

The great metaphors of political theory are deliberate contrivances. In political theory as in ordinary discourse, however, words and meanings are very often taken over from another sphere without the speaker's or hearer's awareness that a transference has taken place. Such usages are referred to variously as "dead" or "fossilized" or "hidden" or "latent" metaphors. (As we see, even the accounts of metaphor are themselves highly metaphorical.) Thus we speak of "Congress" without recognizing that the term referred originally to a meeting, that "court" meant a pen or enclosure, that "to govern" was to handle the rudder or to steer, or that a "minister" was a small man as opposed to a *magister,* a big man. Our language is filled with words whose original reference was to the position of one's body in the spatial-temporal world or to the experience of other bodies. We make use of these words regularly in political speech, as, for example, when we speak of a king ruling "over" his subjects, or the "fall" of a government, or opinions on the "left" or the "right," or an "open" society, or a popular "front," or the "lower" class, or a "hard" decision. These hidden metaphors are found in all languages, and there is some evidence of consistency in the way they are used. Asch (1955) has shown that in languages belonging to different families and far separated in time and space, terms denoting the physical properties of "straight" and "crooked" are used, as in English, to denote honesty, righteousness, and correct understanding, on the one hand, and dishonesty and wile, on the other. We may suppose that people everywhere find it natural to speak of a king ruling "over" his subjects and not "under" them.

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[4] Specifying exactly what happens in metaphorical usage is a more complex task than my analysis might indicate. Writers that undertake this task include Black (1962), Berggren (1962, 1963), Henle (1965), Hesse (1966), Richards (1965), and Sapir (1977).

[5] On metaphors of machine, organism, and servomechanism, see Landau (1972, pp. 78–102), Deutsch (1963), and Campbell (1971). On other types of political metaphors, see Bambrough (1967), Drucker (1970), Kress (1969), Lasky (1970), Lipsky (1976), Rossum (1976), and Zashin and Chapman (1974). For an illuminating study of political metaphor in a nonliterate society, the San Blas Cuna of Panama, see Howe (1977).

Recent proponents of a "tension theory" of metaphor (see Turbayne, 1962; Berggren, 1962) have tended to deny that such usages as these are genuine metaphors at all, since there is no longer a consciousness of tension between a literal and a figurative meaning. Yet it would seem that in most cases of metaphorical usage, we are conscious of the tension or pretense only after the fact, when we look back on the metaphor and interpret it, not when we first use or apprehend it. As Edie points out, we rarely attend to the words we are using when we speak or listen, but rather to the things that the speech is about; therefore, "any apprehension of a verbal opposition or a conflict between the literal and metaphorical senses of the words we are using at any given moment can arise only in later reflection" (1976, p. 263). Most metaphors are "hidden" at first, especially for the hearer.

We are in a position now to identify the fundamental problems that any account of metaphor and political knowledge must face. Let us take notice of these problems, but leave for later a consideration of how they are best resolved. First, there is the problem of why metaphors should arise at all in discourse about political things. Is the cognitive status of these things such that metaphors are needed to identify or discern them? A second problem is to understand the basis for the illumination that metaphor provides. Does it lie in some likeness or proportion that political things bear to things from which metaphors are taken? Is the likeness discovered by the mind or created by the metaphor? A third problem is how to test metaphors or decide on their appropriateness. Are empirical criteria of verification to be used or some other criteria? Finally, there is the problem of whether or not metaphors are indispensable to political knowledge. Is the cognitive import of any metaphor reducible to literal assertions of equivalent meaning? Does political thought go beyond metaphors, or is it necessarily determined and limited by them?

The Verificationist View of Political Metaphor

Deutsch and Landau have provided important statements of what I am calling a verificationist view of political metaphor. They hold that metaphors, when developed as clear and explicit models, can play a vital role in the search for political knowledge, but they insist that metaphors or models must meet a fundamental requirement in order to be acceptable in political science: the analogy or likeness that is presumed to exist between model and thing must be subject to verification. In Landau's words, "to employ a model is *always* to propose the existence of analogy," and this proposed analogy must be "verified through careful analysis and research." The "verification of the proposed analogy" requires that the essential characteristics of the model be made explicit and that points of correspondence between the model and the thing under study be identified. Once the model has been "made operational" in this manner, hypotheses can be drawn from it and subjected to empirical testing. We can determine by sensory observation whether or not political reality has the features that the model predicts. Political science must be suspicious of metaphors which cannot be thus operationalized. These metaphors may be suggestive, but "they provide only a vague *apprehension* of similarity" and not a testable hypothesis. Most "can neither be justified nor invalidated and they remain outside the scientific domain quite impregnable to the tests of experience" (1972, pp. 222–25).

Deutsch reasons in a similar vein. Models must in all cases "be tested for their *relevance:* do they match those aspects of the empirical process in which we are interested to a degree of accuracy sufficient for our purposes?" (1963, p. 11.) The relevance of a model, or its resemblance to the situation under study, "cannot be discovered from the model, but only from a physical process of *verification,* that is, physical operations for matching some of the structure of the outside situation . . . followed by some *critical process,* that is, further physical operations that depend in their outcome on the degree of correspondence between the structure proposed from the model and the structure derived from the outside facts" (1963, pp. 19–20). Models, like all the other conceptual equipment of science, must be tested operationally, i.e., they must "lead to inferences capable of being confirmed or refuted by repeatable physical operations" (1963, p. 4).

As we see, the basic assumption of verificationism is the priority of the facts of political experience to metaphorical speech and thought. Metaphorical insights are treated as hypotheses until such time as they are verified by the observation of political reality itself. Such verification presupposes that knowledge of political reality is available through "physical operations" or empirical testing and also that this knowledge can be obtained independently of the metaphor or model to be tested. If this independent factual knowledge were not available, political scientists would be unable to determine, in the way the verificationist view requires, whether or not their metaphors or models "match reality."

The verificationist is likely to criticize the way metaphors have been used traditionally in political philosophy. Macdonald, for example, grants that analogies are useful in science to discover "verifiable empirical statements" (1965a, p. 102) or to give "new information about empirical facts" (1965b, p. 184), but she denies that they can be used by philosophers to answer questions that cannot be resolved by producing empirical facts. For example, political philosophers have perennially tried to answer the question: "Why should I obey any law or acknowledge the authority of any state or government?" Macdonald believes that the question itself is senseless, because no general criteria of obedience can be found that apply to every instance. Nevertheless, political philosophers have tried to derive a general answer from great metaphors or analogies, such as the idea that obligation rests on a "social contract" or on one's relation to a "social organism" that is higher than any of its individual parts. The contractual and organic theories of obligation, like other philosophical theories that are based on analogy, have no factual or cognitive meaning. They do not convey any genuine information about political affairs. There are no facts which could support the one and refute the other. The difference between these theories is to be found not in their factual meaning but in their practical and psychological effects, such as the attention they might bring at a crucial moment to some political criterion that is being overlooked or denied (1965b, pp. 182, 193–94).

Macdonald's argument raises the question of just what kind of meaning metaphorical expressions can have. It would seem that a metaphorical sentence must be meaningless by strict verificationist criteria, since it is neither a factual nor an analytic statement. Some empiricists have avoided this conclusion by holding that metaphors can sometimes be translated into literal sentences that make either a true or a false assertion. For example, "George Washington is the father of his country" might be paraphrased as "George Washington is a general and statesman who was chiefly responsible for establishing the United States as an independent nation." This approach is open to the charge that the literal paraphrase no longer has the same meaning as the metaphor which it replaces. At this point, a distinction might well be introduced between "cognitive" and "emotive" meaning. If "George Washington is the father of his country" means more than our literal paraphrase, then the residue will be understood as emotive meaning. The noncognitive meaning components of the sentence will be taken as expressing the speaker's emotions or feelings towards Washington and perhaps as intending to evoke similar emotions in the hearer. The emotive meaning of a metaphorical sentence would not be regarded as a proper part of its scientific meaning, although emotive meaning might be given a legitimate place in other types of language, such as poetry.[6]

There appears to be a difference at least in emphasis between Deutsch and Landau as to how metaphorical models can be useful in scientific inquiry. While Deutsch seems willing to grant that a model can provide an "explanation," at least in a qualified sense of the term (1963, pp. 8–9), Landau speaks of the heuristic value of models in what has been called "the context of discovery" (1972, pp. 221–22). Whether the differences between them are substantial or not, this matter has been a point of great controversy in the empiricist tradition; and it bears decisively on our inquiry into the contribution of metaphor to political knowledge.

Some writers—probably those who have influenced political science most in recent decades—follow such scientists as Duhem in denying that physical analogies play an essential role in explanation (see Duhem, 1954, pp. 55–104; Nash, 1963, pp. 230–53; Hesse, 1966, pp. 1–56). According to Hempel, for example, science explains phenomena by subsuming them inferentially under principles that have the character of general laws. Detailed analogies or models may prove useful in the search for new explanatory principles, especially when they are based on "nomic isomorphisms," or similarities between the laws governing one set of phenomena and those governing another set. Nevertheless, an analogical model itself explains nothing, and "all references to analogies or analogical models can be dispensed with in the systematic statement of scientific explanations" (1965, p. 44; see also pp. 433–47). Rapoport reaches a similar conclusion by distinguishing sharply between the analogical or metaphorical models that social scientists often use and the mathematical models of the exact sciences. The mathematical model posits specific relations among specific variables, and these can be translated into verifiable assertions about the observable world. Its underlying assumptions are kept as simple as possible, and the "reality" of these assumptions is frequently a matter of indifference. The analogical model, by contrast, bases its claim to explanatory value on "the appeal to see in a seeming similarity of two phenomena the explanation of the unfamiliar one in terms of supposed understanding of the familiar one" (1959, p. 354). Social scientists who base their theories on such models try to support them by marshaling factual material "in such a way that the reader who views this 'evidence' through the metaphors, concepts, and definitions of which the 'theory' is constructed will have the experience of 'understanding' " (1963, p. 80). Rapoport denies that metaphors and analogies can be accepted as scientific explanations. At best, they are sometimes helpful in preparing the mind to make more precise investigations and eventually to construct the sorts of proofs that mathematical rigor demands (see 1963, p. 80).

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[6] The interpretation of metaphorical meaning as emotive meaning is associated primarily with I. A. Richards. For Richards' views on emotive language and metaphor as they developed over several decades, see Schiller (1969, pp. 48–88).

Other writers have assigned a quite different role to physical models or analogies in scientific explanation. Campbell, following such nineteenth-century physicists as Lord Kelvin, has argued that analogies, far from being mere aids to the formation of hypotheses or theories, are "an utterly essential part of theories, without which theories would be completely valueless and unworthy of the name." In Campbell's view, it is never difficult to find a formal or mathematical theory to explain physical laws in a logical way. What is difficult "is to find one which will explain them logically and at the same time display the requisite analogy" (1953, pp. 297–98; see Hesse, 1966, pp. 1–56). This position is developed today primarily by antipositivist philosophers of science such as Toulmin. Toulmin maintains that a theory is never felt to be entirely satisfactory in the physical sciences unless its mathematical calculus is supplemented by an intelligible model. A mathematical calculus may be an excellent way of expressing the relationships of phenomena, but to understand them, to "see" their connection, a model is necessary. The scientist must be able to look at the phenomena with a professional eye "and 'see' their behaviour in the way his theories require: this . . . is the purpose of using models in the physical sciences" (1965, p. 165; see also pp. 34–39, 164–67).

The Constitutivist View of Political Metaphor

The constitutivist view of political metaphor is more often implied than developed in the writings of political scientists, but its essential features are brought out clearly by Edelman (1971, pp. 65–72). Edelman puts emphasis on the complexity and ambiguousness of events in the empirical world of politics and the elusiveness of causes and remedies. For the mass of people at least, the bewildering political universe would be altogether unintelligible if it were not ordered and given meaning by language. Drawing upon some recent theories of symbolism, Edelman maintains that "language does not mirror an objective 'reality,' but rather creates it by organizing meaningful perceptions abstracted from a complex, bewildering world." Political reality is not something given to observation. It is constituted by language through the creation of shared meanings, perceptions, and reassurances. Metaphor plays a crucial role in the constitution of political reality: "Thought is metaphorical and metaphor pervades language, for the unknown, the new, the unclear, and the remote are apprehended by one's perceptions of identities with the familiar." People are led by internal and external conflicts and passions to attach themselves to one or another of the range of metaphors and myths that are available for understanding a particular political situation. Once chosen, a linguistic form shapes one's view of political reality by intensifying some perceptions and screening others out of attention.

Edelman uses some metaphors of war to illustrate his contention that metaphors define the alternative patterns of perception to which people respond: "To speak of deterrence and strike capacity is to perceive war as a game; to speak of legalized murder is to perceive war as a slaughter of human beings; to speak of a struggle for democracy is to perceive war as a vaguely defined instrument for achieving an intensely sought objective." In each case, the metaphor creates political reality by shaping perceptions of both facts and values. The metaphor can be "a subtle way of highlighting what one wants to believe and avoiding what one does not wish to face." It seems doubtful that a metaphor can, on Edelman's view, be verified or falsified by perception, because perceptions of political reality are determined by the metaphor: "Once accepted, a metaphorical view becomes the organizing conception into which the public thereafter arranges items of news that fit and in the light of which it interprets the news. In this way a particular view is reinforced and repeatedly seems to be validated for those whose attitudes it expresses." A dominant metaphor thus tends to become self-perpetuating, although competing metaphorical definitions of political reality can arise to supplant it and create a new orthodoxy.

Edelman provides a good concise statement of the constitutivist view of metaphor, but it is necessary to look outside of political science for comprehensive accounts. I shall draw on recent works on metaphor by Wheelwright, Turbayne, Edie, and Black to illustrate the main principles of a constitutivist view and to show how these differ from verificationist principles. My intention is to characterize a typical view, not necessarily one that each of these writers would agree with completely.

For the constitutivist, metaphors are neither linguistic ornaments to be viewed with suspicion nor heuristic devices to be discarded once objects are known. They are indispensable components of language and—since thought depends on language—indispensable components of thought as well. As Wheelwright explains, "thought is not possible to any significant degree without language, nor language without metaphoric activity whether open or concealed" (1962, p. 128). Metaphors are as inescapable in science as in poetry or theology, and they cannot be reduced to literal statements without loss of cognitive meaning.

The heart of the constitutivist view of metaphor is its claim that reality as we know it is constituted by metaphorical language and thought. While there is not full agreement on the ground and implications of this claim, constitutivists are likely to defend one or more of the following propositions. First, they deny that the grouping of things into different sorts or kinds is something fixed by nature or even something built into the structure of human thought. There is an ultimate arbitrariness, therefore, in the metaphorical presentation of the facts of one sort as if they belonged to another. Neither the sorting of facts that we begin from as the literal base of our metaphor nor the sorting to which this literal meaning is transferred has any necessity about it. In Turbayne's words, we "cannot help but allocate, sort, or bundle the facts in some way or another," but any particular allocation or sorting is "perfectly arbitrary." It is true that names or categories are already there for us when we first begin to speak, but these are "part of our cultural, not our natural, heritage" (1962, pp. 64–65, 79–80). Some assumption such as this seems to lie behind Black's (1962) critique of what he calls the "substitution" and "comparison" views of metaphor and his defense of an "interaction" view, which interprets metaphor as both the transfer to a principal subject of the "commonplaces" that a given culture associates with a subsidiary subject and an alteration in the way the subsidiary subject itself is seen. To give Black's example of this alteration in perception, calling a man a wolf not only puts man in a special light, but also makes the wolf seem more human than he otherwise would.

In the second place, constitutivists are apt to hold that the metaphors which are most important to our thinking involve the creation rather than the discovery of likeness. Black had suggested this possibility in criticizing the "comparison" view: "It would be more illuminating in some of these cases to say that the metaphor creates the similarity than to say that it formulates some similarity antecedently existing" (1962, p. 37). Wheelwright made it the basis for a distinction between two types of metaphor, which he calls "epiphor" and "diaphor." Traditionally, metaphor had been understood to rest on the discovery of some real similarity that unfamiliar or obscure things bear to things of another sort that are more accessible to knowledge. Wheelwright speaks of this as "epiphor." While epiphor extends meaning from the familiar to the unfamiliar by way of some presumed likeness, "diaphor" juxtaposes the particulars of experience in such a way as to create new meaning and a lively sense of the presentness of objects that possess an irreducible core of mystery (1962, pp. 70–91). Edie follows Black and Wheelwright in emphasizing the importance of the type of metaphorical usage "which *creates* and *brings similarities to be* rather than merely formulating them" (1976, p. 191).

Finally, constitutivists are likely to doubt that reality has an intelligible structure apart from the organization that metaphorical language imposes upon it. According to Wheelwright, reality is "latent, subtle, and shy." It is "unwilling to give up its innermost secrets even to the most enterprising explorer" (1962, pp. 172). Man gropes through metaphorical language to express the tensions that he feels in existence, to "give some hint, always finally insufficient, of the turbulent moods within and the turbulent world of qualities and forces, promises and threats, outside him" (1962, p. 46). Objects have a mysterious presence about them that cannot be captured by explanations and theories that treat them as spatio-temporal things (1962, pp. 158–59). For Edie, it is through the invention and use of language and expression generally that "man is enabled to orient himself in the confused world of lived experience" and "to put order into his experience of the world" (1976, p. 161). The meaning of the world is constituted intersubjectively through language.

This line of reasoning suggests that human thought cannot hope to go beyond metaphorical perspectives to reach a true and comprehensive understanding of things as they are. Not all constitutivists would be willing to defend such a conclusion, but Wheelwright and Turbayne, at least, embrace a thoroughgoing perspectivism. Wheelwright puts the point this way:

> To think or speak about reality is always to do so through one perspective rather than another, and to compare one perspective with another must involve the adoption of a third perspective which will be only partly pervious to them both. Thus reality as a whole cannot be typed, for to type it is to limit it to an arbitrarily chosen perspective (1962, p. 170).

No single perspective can claim to give the true understanding of reality or, indeed, to be superior to other perspectives for all occasions. The best we can hope to do is to

> catch partisan glimpses, reasonably diversified, all of them imperfect, but some more suited to one occasion and need, others to another. If we cannot hope ever to be perfectly right, we can perhaps find both enlightenment and refreshment by changing, from time to time, our ways of being wrong (1962, pp. 172–73).

Similarly, Turbayne denies that we can go beyond metaphors to grasp the literal truth of things: "We cannot say what reality is, only what it seems like to us, imprisoned in Plato's cave, because we cannot get outside to look. The consequence is that we never know exactly what the facts are. We are always victims of adding some interpretation" (1962, p. 64).

The key differences between the verificationist and constitutivist views of political metaphor should now be apparent. Verificationism holds that any claim of metaphors, analogies, or models to have cognitive meaning must be tested by a factual knowledge of political reality. It assumes that the political facts, as we know them from sense perception, are somehow prior to and independent of metaphorical speech and thought. Constitutivism denies that political reality can be recognized or its meaning understood independently of some metaphorical interpretation. One's understanding of the facts is shaped by the metaphor, so the facts cannot provide an independent means of verifying or falsifying the metaphor. Black (1962, pp. 39–47) invites us to think of a metaphor as a filter or screen which selects, organizes, and transforms what we see. Turbayne (1962, pp. 22, 66) draws out the radical implications of this position. It is never possible, he maintains, to say what the facts *really* are. A metaphor is like a screen or filter between the mind and the world. Once we see the world from the point of view of a particular metaphor, the face of it is changed. Adopting a new metaphor changes our attitude toward the facts.[7]

On Four Problems of Political Metaphor

The manifestationist view that I am proposing finds important elements of truth in both the verificationist and the constitutivist views of political metaphor, but it also sees fundamental difficulties in each. Verificationism retains the principle—vital to a science of politics—that political reality is something discovered rather than something created by the knower. Yet it fails to recognize that metaphor is essential to political knowledge, because it believes that the meaning of political reality can be found in what is observable to the senses independently of metaphors. Constitutivism recognizes that metaphors are essential to political knowledge, but it gives up the possibility that political inquiry can move beyond metaphorical perspectives to a true understanding of political things. The view that I am proposing agrees with verificationism that political reality has an intelligible structure that is not simply the creation of metaphorical language and thought. It agrees with constitutivism that metaphor is necessary to disclose the meaning of political things, since these things are not given directly to observation. Metaphor discloses the meaning or reality of political things not by constituting them, however, but by manifesting their intelligible structure. The best way to explain this manifestationist view is to show how it would resolve the four central problems of metaphor and political knowledge that we identified earlier.[8]

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[7] Black's critique of Whorf's linguistic relativity (1962, pp. 244–57) indicates that he would not wish to accept Turbayne's radical conclusions.

[8] After my essay on political metaphor had been completed, Ricoeur's *The Rule of Metaphor* (1977) was brought to my attention by Fred Dallmayr. This valuable work examines the major alternative accounts of metaphor, both classical and modern, in great detail and with a fine sensitivity to the philosophical issues

Why is political speech metaphorical? Since metaphor is transference of meaning from the familiar to the unfamiliar, the pervasiveness of metaphor in political speech is a sign that political things are somehow less familiar of accessible than the things from which political metaphors are taken. Yet what is the reason for the obscurity of political things? Why do we rely on metaphors to make them manifest?

The obscurity of political things arises in the first instance from the fact that they cannot be observed directly by the senses. It is simply impossible to perceive the political in the immediate or direct way that we experience bodies and their sensible qualities, including our own bodies. Insofar as political things have an embodiment, that bodily aspect is perceptible, but the shapes and qualities that are given to the senses can always be accounted for as something in themselves, so that there is no necessity to see them as something else, something political. We like to speak of observing political behavior, but all that we can actually perceive with the senses are motions of limbs, facial expressions, articulate sounds, and artifacts of one sort or another. Strictly speaking, therefore, political things are unobservable things. The sensible contributes to political knowledge only insofar as it takes us beyond itself in one way or another to the unobservable. Verificationism is caught in this dilemma: if it interprets strictly its principle that politics is an empirical process, available to physical operations, it can never reach the political at all; if it interprets the principle loosely to mean only that the political is somehow inferred from the sensible, it undermines its theory of meaning and its distinction between facts and values.

Metaphor is a way of moving from the observable or sensible to the political. As we have seen, political metaphors very often involve a transfer of words and meaning from some object or activity that is accessible to sense perception. Somehow this transference serves to make manifest or bring to presence the unobservable things.

It is not in political discourse alone that the unobservable is named by reference to the observable. Whenever we speak of the relationships of human beings, of their inwardness or soul, and of their place in the encompassing whole, we express ourselves in metaphors that are drawn overwhelmingly from the sphere of observable and tangible things. Usually we are unaware of just how metaphorical this discourse is, because the metaphors are mostly hidden or latent. Thus Locke deems it important in understanding the origin of knowledge to notice

> how great a dependence our words have on common sensible ideas; and how those which are made use of to stand for actions and notions quite removed from sense, have their rise from thence, and from obvious sensible ideas are transferred to more abstruse significations, and made to stand for ideas that come not under the cognizance of our senses; v.g. to *imagine, apprehend, comprehend, adhere, conceive, instil, disgust, disturbance, tranquillity,* &c., are all words taken from the operations of sensible things, and applied to certain modes of thinking. *Spirit,* in its primary signification, is breath; *angel,* a messenger: and I doubt not but, if we could trace them to their sources, we should find, in all languages, the names for things that fall not under our senses to have had their first rise from sensible ideas (1959, Vol. 2, p. 5).

Vico, in a much fuller and more complex account of the metaphorical origins of thought and language, agrees that people have understood "spiritual things," such as the faculties of the mind, the passions, virtues, vices, sciences and arts, through metaphors formed from their experience of bodily things. Vico holds it to be

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that metaphor raises. If I read him correctly, Ricoeur develops a foundation for what I have called a "manifestationist" view of metaphor. He reaffirms, against its modern critics, the Aristotelian principle that metaphor is grounded on resemblance, or on the intuitive perception of a real similarity in dissimilar things. This resemblance guides and produces the metaphorical statement. It is not simply fashioned by the statement, as Black, Wheelwright and others have argued (Ricoeur, 1977, pp. 191–200). Furthermore, the meaning of metaphorical language is not reducible to "emotive" meaning, as empiricist theories often hold. Metaphorical language says something about reality. It can make reference to things in such a way as to be true or false, although metaphorical truth cannot be determined by techniques of empirical verification (pp. 216–56). In its subtitle, Ricoeur refers to his book as a study of "the creation of meaning in language." He makes it quite clear, however, that the creation of meaning or insight by metaphor is linked inseparably to the discovery of the nature of being (pp. 306–09). The conditions for linguistic reference in general, and thus the meaning of language as a whole, are set by "the relation of language to its counterpart, reality" (p. 303). Semantic innovation organizes reality in certain ways, but it also "[makes] manifest a way of being of things" (p. 239). Moreover, "the referential field can extend beyond the things we are able to show, and even beyond visible, perceptible things" (p. 298). My account of political metaphor begins from principles such as these.

"the universal principle of etymology in all languages" that "words are carried over from bodies and from the properties of bodies to express the things of the mind and spirit" (1948, pp. 70, 114–19).

The legitimacy of this metaphorical extension of names and meaning has been widely questioned since the time of Locke and Vico by a tradition that includes Bentham, Comtean positivism, the behaviorism of Watson and his followers, and logical positivism. This tradition regards the use of metaphors to speak of what Vico calls "spiritual things" as an abuse of language. Scientific thought, which understands the meaning of things by reference to observables, is seen as an unqualified advance beyond theological and metaphysical stages of thought. It is true that logical positivists moved toward a more liberal criterion of meaningfulness which could accommodate the fact that theoretical physics makes extensive use of concepts which do not refer to anything directly observable. As Carnap pointed out in this later period, "the prodigious growth of physics since the last century depended essentially upon the possibility of referring to unobservable entities like atoms and fields" (1963, p. 80). Accordingly, Carnap developed the view that theories must be tested indirectly by linking theoretical concepts to observable things by "correspondence rules" (1963, pp. 77–81; 1966, pp. 225–46). Carnap's later view is possibly liberal enough to sanction metaphorical references to "nonobservables" in the human sciences, but not all empiricists would be this generous. Brodbeck, for example, grants that references to invisible entities cannot be eliminated from atomic physics, but she denies that social scientists are warranted in introducing similar concepts. In this respect, social science is like classical Newtonian theory: all of its concepts refer to what can be observed, namely, to the "observed behavior" of individuals or groups. Social scientists should not "hunger after the complexity of the invisible," because the phenomena they study are not of "atomic or subatomic dimensions." Thus the notion of "model" appropriate to these nonobservable entities has "no intelligible meaning in social science" (1959, pp. 399–401).

The contrary view that I am proposing affirms the reality of nonobservables in the sphere that is of concern to political inquiry and the indispensable role of metaphors in making these things manifest. The shapes and qualities of sensible things are not all that we can know, but they are what we discern first with the greatest distinctness and clarity. Since we name as we know, it is not surprising that the words used in political discourse most often had some early reference to sensible things. It is natural that as we extend our experience and thinking to discover things not given directly to sense perception, we should name and understand them by analogy to those sensible things that are discerned first with greater clarity. Political metaphors thus reflect the trajectory of our knowledge from the observable to the unobservable, or from what is less obscure to us to what is more obscure.[9]

I do not wish to contend that political metaphors consist exclusively of a transfer of names or meaning from the sensible to the unobservable. Such might be the case when, for example, we speak of a political structure as a pyramid or a machine, but it is not the case when we speak of political relationships in terms of more familiar human relationships such as fatherhood. Fatherhood may disclose itself more fully to sense perception than political ruling, but something more than sensible meaning is transferred when we call a ruler a father. In other words, unobservability is one reason, but not the only reason, for the relative unfamiliarity of political things. Relationships such as fatherhood also have an unobservable dimension, but they may nonetheless be more familiar to us than political relationships—and thus fitting sources for political metaphor—because of their proximity or simplicity.

The fact that we sometimes name and try to illuminate things by metaphors drawn from the political sphere indicates that there are unobservables which are even more obscure to our vision than political things. For example, while bodies and their motions are visible to the eye, their originating and sustaining principles are not. Terms such as "cause" and "law" are taken over from the political sphere to refer to these first principles, not only by theological accounts of the world but also by scientific accounts. Even the word "thing" seems to have referred originally to a political assembly or court of law. Many writers have used political metaphors to characterize the inward nature or soul of the human being. Hume declares that he "cannot compare the soul more properly to anything than to a republic or commonwealth" (1955, p. 261; see also pp. 415–16). It is in Plato's *Republic* that we find the most extensive effort to understand the soul by reference to the city and the types of regime.

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[9] For a similar argument, see McInerny (1961, pp. 54–56; and 1968, pp. 21–24).

Socrates justifies this procedure by asserting that the city is bigger and easier to observe than the soul.[10] As Rosen points out, this Socratic approach rests on the assumption that our eyesight is not keen enough to see the *psychē* itself: "We turn therefore toward the city, within which the *psychē* becomes visible. The articulated structure of the city, the structure which both differentiates and integrates the possible modes of experience, is the mirror-image of the *psychē*" (1961, p. 137). Political metaphors are useful in psychology because the *psychē* is even less familiar or accessible than political things.

How do political metaphors manifest things that are not observable directly? In considering how metaphors can make unobservable things manifest, it is necessary to distinguish between metaphorical naming and metaphorical reasoning. Naming unobservables is the first step towards identifying and coming to know them. In the initial stage of metaphorical discovery, we reach out to identify something as yet hidden or indistinct by giving it a name. Since the thing has no name of its own, one is transferred to it from a familiar and accessible thing which is somehow like the unnamed thing. Naming the thing serves to bring it out into the open, into the public domain, so that it can be discerned and discussed. Sometimes the metaphorical discovery of political things is a case of coming to know something of which we had no prior experience or awareness. At other times, it is a case of discerning more clearly something of which we had already some impression, some vague and fragmentary experience.

When we lose sight of the reality to which a political term refers, our discernment can sometimes be sharpened by locating the hidden metaphor on which the term is based. Etymological inquiries can be of help in retracing and reliving the original transferences by which the human things came to be identified. As Austin has observed (1961, p. 149), a word hardly ever shakes off its etymology and its formation. Its older meanings persist in spite of all changes in and extensions of and additions to its meaning. It is necessary to remember, however, that there is a difference between the etymology of a word, or that from which it is derived or transferred, and its signification, or that which it is intended to signify (see McInerny, 1968, pp. 12–24).

Metaphorical reasoning goes beyond naming in its sustained effort to infer the characteristics of a thing from something that resembles it. Metaphors that lend themselves to systematic exploitation by scientific reasoning are now typically referred to as "models." It is important to notice, however, that not all models involve a transfer of meaning of the type that we are discussing. For example, "model" sometimes refers to an exemplar or ideal, to that which is perfect of the kind, whether real or imaginary. This is a nonmetaphorical model, because no sort crossing is involved in its application. A concrete thing that helps us to understand something else of the same kind might also be considered a model. Thus in comparative politics, one might predict that property *d* will be found in party system *Y*, since *d* is found in system *X*, and *Y* is like *X* in properties *a* and *b*, which tend to cause *d*, although different in property *c*.[11] Reasoning of this type is not metaphorical in the sense that we have been discussing, because the same kinds of things are being compared. As we have seen, some writers deny that a metaphorical or analogical relationship is involved in mathematical models, although the more common practice is to say that these involve "formal" analogies as distinct from the "substantive" or "material" analogies that can exist between real objects (see Nagel, 1961, p. 110; Hesse, 1966, p. 68).

Verificationism is correct, I believe, in grounding metaphorical reasoning on a likeness or similarity that is discovered in things and not created by language. In some cases, the thing from which our reasoning begins has properties that are similar to those found in some less familiar thing of another kind that we are attempting to understand. After establishing that the things are similar in some respects, we may reason or predict that they are similar also in respect to a property known to belong to the familiar thing but not yet known to belong to the other.[12] In other cases, the things are similar not so much in properties as in some proportion. A familiar political metaphor takes this form when we reason: father is to child as ruler is to country. When the metaphor is stated this way, the similarity to which we draw attention is not primarily between corresponding properties of father and ruler or child and country, but between the relationships of father to child and ruler to country.[13] Metaphors with at least four terms, having an equality or similarity of proportions between each pair of terms, are analogies in the classical Aristotelian sense.

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[10] *Republic* 368 c–369 a.

[11] This corresponds to Hesse's Example A of analogical reasoning (1966, pp. 58–59).

[12] Compare Hesse's Examples B and C of analogical reasoning (1966, pp. 60–62).

While verificationism properly grounds political metaphor on a discoverable likeness, it cannot give an account of this likeness on empiricist principles. These principles enable us to understand how one observable thing can be like another in sensible qualities, tangible structure, behavioral patterns, or external relationships. When political metaphors begin from observables, however, the problem is to explain how an observable thing can be like something unobservable that is not simply a microscopic body.

Unobservables cannot be like observables in sense qualities or, if they are incorporeal, in qualities that are proper to bodies, but they may share with observables the likeness that is possible between things that fall under some common principle or concept. We know, for example, that a great many of the terms that are used to characterize intellectual activity are derived metaphorically from the activity of seeing with the eye ("observe," "see," "intuit," "inspect," "view") or from optical phenomena ("reflect," "focus," "outlook," "illumination," "perspective"). Seeing with the eye is a sensory activity and knowing with the intellect is not, but they bear the likeness of activities that have a principle in common. The principle common to both activities is that of cognition. Each activity is a form of cognition. It is appropriate, therefore, that we should transfer names and meaning from the more familiar activity of seeing to rational activity and even try to give an account of the intellect by analogy to the perception of visible shapes. Metaphors of sight are able to make manifest the nature of intellectual knowledge by way of the common principle of cognition or discernment.[14]

I suggest that political metaphors work the same way when they move from the observable to the unobservable. To take some classic examples, Plato manifests the nature of justice by analogy to other activities that fall under the principle of "minding one's own business." Everyone can see what it means for a craftsman to mind his own business, or for each part of some heterogeneous whole to perform its work well, and this insight can be carried over to the elucidation of justice in the city and in the soul. Machiavelli's metaphor of the lion and the fox teaches us that rulers can achieve their purposes by recognizing dangerous situations before it is too late to escape harm and by standing courageously against adversaries. This insight is possible because cleverness and courage are *principles that manifest themselves in the behavior of foxes and lions as well as in human actions.* At the time of our nation's founding, James Wilson used the metaphor of the pyramid to show that a durable government must have a broad foundation in the consent of the whole people (see Rossum, 1976). This metaphor helps us to understand what stability might mean in a political structure by reference to the principle of stability in a physical structure. When a metaphor or model of political activity begins from observable things, its intelligibility depends on some similarity between the observable and the political that is possible because they exemplify a common principle.

How are political metaphors tested? A metaphor gives us some insight into its subject at the moment we understand it, and some might argue that this insight, this sense of illumination, is sufficient in itself to verify the metaphor. Political inquiry cannot be satisfied with this solution. The insight that the metaphor intends must be regarded as a claim to truth or as an opinion to be tested.

Constitutivist views have great difficulty in saying how political metaphors should be tested. By arguing that political reality is constituted by metaphorical language, so that our perception of reality must be congruent with our particular metaphorical outlook, constitutivism eliminates the possibility of testing political metaphors by an independent grasp of what is real. It tends to rely on other ways of judging their appropriateness, such as whether or not a metaphorical outlook fills personal needs. Verificationism quite properly emphasizes that political metaphors must be tested so as to determine the truth or falsity of the view they provide of political reality, but it is wrong in thinking that this can be an empirical test. Since political things are not given to sense perception, some type of nonempirical verification must be devised for political metaphors.

Political metaphors manifest by way of

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[13] See Hesse's Example D of analogical reasoning (1966, pp. 62–64). I do not agree with her contention that this type of reasoning is "persuasive rather than predictive."

[14] Compare the view that analogy rests on a *ratio communis,* or common notion (see McInerny, 1961, pp. 144–51; 1968, pp. 78–81).

likeness—the likeness that is possible not only between different kinds of things, but also between observables and unobservables. We test a political metaphor by determining if the political thing is like the nonpolitical thing in the way that the metaphor intends. Since metaphors always involve an element of falsehood, a political metaphor cannot be falsified by extending it beyond the intended likeness. Some might try to reduce our metaphors to absurdity by pointing out the many ways in which rulers are not in fact like fathers or foxes or lions, but they would only succeed in showing that these are genuine instances of sort crossing or transference of meaning from one kind of thing to another. Sort crossing always presupposes fundamental differences as well as likenesses (see Turbayne, 1962, pp. 56–60).

The first step in testing a metaphorical insight is to articulate it clearly as an assertion. This articulation will typically be in terms other than those of the metaphor itself. For example, the metaphor that likens rulers to fathers can be articulated as the assertion that it is the nature of political rule to look to the good of subjects. Machiavelli's metaphor of the fox and the lion can be articulated as the assertion that successful rule depends on the right combinations of cleverness and courage. This procedure is open to the obvious objection that what we are testing is not the metaphor itself but a paraphrase. The objection can be met by distinguishing between the truth of a metaphor and its adequacy as a metaphor. In testing the truth of a metaphor, we typically begin by articulating it in other terms as an assertion about reality. Adequacy, however, has to do with the irreducible features of a metaphor, those that are lost in a literal restatement. Adequacy cannot be divorced completely from truth, but it is determined mainly by criteria of style and taste, such as liveliness, freshness, or appropriateness.[15]

Once the insight that a metaphor intends has been articulated as an assertion, we proceed to test it as we would any opinion about political things. Constitutivism is undoubtedly correct in holding that a political metaphor gives us a perspective, a point of view. The question, however, is whether we are compelled to see political things from one or another partial and imperfect perspective, or whether the various perspectives contain some genuine insight into the nature of the thing they view. Sense perception gives us only perspectives of an observable thing, but these are attended by some sense of the thing in its wholeness. By the same token, the doxic perspectives (if we may coin a term from *doxa,* opinion) that metaphors contain are attended by some grasp of the what-is of the thing they bespeak. Political inquiry is the attempt to move from perspectives to an integral view, from opinions to a grasp of the thing in its wholeness. It proceeds by taking a variety of opinions or viewpoints into account, weighing them against each other, and seeking the comprehensive view that can withstand criticism. In the course of inquiry, there is a growing awareness of the shape of things as a whole, and this awareness helps us increasingly to see the partiality and distortion of the perspectives from which we began. We perceive order in experience whose meaning was vague and inchoate at first. Things fall into place. Even if inquiry does not terminate in a view of things that we can accept as comprehensive and complete, it carries with it the assurance of progress toward such a view.

In sum, political metaphors, or the opinions they embody, are tested in the course of inquiry against each other, against such experience as we have of political life, and against the integral view of things that gradually becomes manifest as inquiry proceeds. We do not see the whole truth about political rule when we see the ruler as a father or a lion and fox or a pilot, but each perspective is likely to contain a part of the truth that must be accommodated by an integral view. The closer we approach an integral or comprehensive view, the more fully we are able to appreciate both the truth that each metaphor makes manifest and the partiality of that truth. Political experience whose significance the metaphors have brought to light can now be used to test metaphorical perspectives and ascertain their limits.

Are metaphors indispensable to political expression and political knowledge? There are several ways to resolve this problem, and each solution finds some support in writings on metaphor. One possibility is to maintain that metaphors are unnecessary in speaking as well as in reasoning about politics. Such a position as this would be difficult to maintain, however, given the pervasiveness of metaphor in political speech and writing. Even the apparent enemies of metaphor, such as Hobbes, find it impossible to speak comprehensively about politics without using them. A political metaphor can often be restated in other terms, but it is difficult to avoid some metaphor in the paraphrase. Latent or hidden metaphors can easily be located in political speech when no overt ones are visible.

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[15] When Aristotle observes that metaphors must be neither far-fetched nor obvious and that metaphorical words set things more vividly before our eyes if they suggest activity, he is dealing with what I would call the adequacy of a metaphor (see *Rhetoric* 1411b 21–1412a 23).

Another possibility is to grant that metaphor is indispensable to political expression, but to deny that our knowledge of politics must be metaphorical. In effect, this position questions the assumption that if we speak in metaphors, we must be thinking that way also. It regards language as having limitations that do not pertain to knowing. I am inclined to think that most of our political knowledge is nonmetaphorical in character, even though we might be able to express it only in metaphorical terms. It is possible to grasp the nature of something political as it *is* in itself. In expressing what we have thus comprehended, it may be necessary to say what the thing is *like.* The position that I am sketching is able to accommodate the undeniable role that metaphors play in the discovery or attainment of political knowledge. Metaphorical thinking might be compared to climbing a ladder. We need it to reach our objective, but the objective, once attained, can be held without it. A common experience in the progress of knowledge is that of looking back and finding that metaphors once necessary to our comprehension no longer are so. Metaphors are indispensable as we reach out to try to understand political things, but it is often possible to leave the metaphors behind as we come to know political things.[16]

The remaining possibility—that knowledge of politics as well as political expression is unavoidably dependent on metaphor—is not entirely wrong. One need not believe that political reality is constituted by metaphorical language in order to conclude that some political knowledge is unavoidably metaphorical. Perhaps there are some dimensions of reality which, though quite intelligible in themselves, are simply beyond our power to comprehend except in metaphorical terms. Lewis observes that "for all of us there are things which we cannot fully understand at all, but of which we can get a faint inkling by means of metaphor." A nonmetaphorical knowledge of these things is not a possibility for us: "We are here entirely at the mercy of the metaphor" (1969, p. 254). While political inquiry can often move beyond its metaphorical beginnings, it may be that we are unable to think about the most comprehensive questions concerning man and politics except in terms of metaphor. When Plato comes to speak in the *Republic* of man's relationship to the political community and to the whole of being, he develops a great metaphor—the allegory of light that culminates in the parable of the cave. He gives us an inkling of what may not be thinkable apart from some metaphor.

Conclusion: Metaphor as a Method of Political Inquiry

When Hobbes likens scientific method to a pathway, he is only making explicit the metaphor that lies hidden in the word "method." Originally, method is *meta-hodos,* a "way after" or "path beyond." We have tried to understand the kind of pathway that metaphor itself provides for political inquiry.

As we have seen, metaphorical transfer is a method or way that is common to all forms of discourse and inquiry about politics. It is not just one technique of investigation among others, a way that political scientists may follow or turn away from at their pleasure. Metaphor is among those operations, such as sorting particulars into kinds and drawing inferences from the regular association of dissimilar things, that are essential to political inquiry.

It is true, of course, that all searching for knowledge shares with political inquiry this dependence upon metaphor. Attention to metaphor would thus afford a profitable avenue for exploring the relationship of political knowledge to scientific or philosophical knowledge of all kinds as well as to poetic or literary imagination. The principal concern of this essay, however, has been to see how the political uses of metaphor illuminate the character of political knowledge itself.

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[16] As Ricoeur explains this point (1977, pp. 300–03), one passes from metaphor to concept as one moves from the imaginative order to the intellectual or speculative order. Metaphorical discourse attains "same" only as "similar," because it rests on images, predicative assimilations, schematizations and imaginative illustrations of various sorts. In the horizon opened up by speculative discourse, however, " 'same' grounds 'similar' and not the inverse" (p. 301). In other words, the concept is able to grasp the identity of the what-is. The conceptual order "is able to free itself from the play of double meaning and hence from the semantic dynamism characteristic of the metaphorical order" (p. 302). Of great significance for political inquiry, I believe, is the mode of discourse that Ricoeur calls "interpretation." Interpretation functions at the intersection of the metaphorical and the speculative domains: "It is a composite discourse, therefore, and as such cannot but feel the opposite pull of two rival demands. On the one side, interpretation seeks the clarity of the concept; on the other, it hopes to preserve the dynamism of meaning that the concept holds and pins down" (p. 303). One might say that interpretation struggles for the univocity of the concept without denying experiences that can find expression only in the metaphorical mode.

Metaphor is essential to political inquiry, because it permits us to extend our knowledge from our familiar world to a region that is not given to immediate experience. This conclusion finds support in the writings of some of the phenomenologists who build upon Husserl's notion of the "life world." Schutz (1973), for example, describes "the world of everyday life" as the location of my experience of sensory qualities, of spatial and temporal relationships, of making and doing, of "cultural objects," and of the cogitations of other human beings. According to Schutz, the world of everyday life is the primary world in which I dwell, but I can experience realities or provinces of meaning that transcend it. He lists the world of politics among these transcendent provinces, along with such provinces of meaning as natural science, art, religion and philosophy. Of particular interest to us is Schutz's contention that we apprehend the phenomena that transcend the world of everyday life in a way analogous to the familiar phenomena within it. Politics and the other transcendent provinces of meaning are revealed by analogical apprehending, or by what I have called metaphor.

Schutz's account helps us to see why the verificationist view of political metaphor cannot be correct. Metaphor is necessary to political knowledge precisely because the meaning or reality of the political world transcends what is given to observation. This transcendence makes it impossible for us to determine merely from observation whether or not there is such a likeness between the nonpolitical and the political as a metaphor supposes. It is true that political inquiry must begin from what can be observed by the senses, but the observable is only a sign of the political—a sign that indicates the existence of political things through association or illuminates their meaning through likeness. The observable can signify the meaning of the political, but it does not contain or enclose that meaning within itself. Schutz points out that not even the world of everyday life is given fully to perception. Our experience of material things as cultural objects, of the inner lives of other persons, and of physical bodies in their wholeness depends in part on the indirect cognition through signs that Husserl calls "appresentation."

Schutz helps us to see also what is questionable about the constitutivist view of political metaphor, although this is not his intention. His account proceeds on the same assumption as constitutivism, namely, that "it is the meaning of our experiences, and not the ontological structure of the objects, which constitutes reality" (1973, p. 341). This implies that the political world, like other transcendent provinces of reality, has no intelligibility apart from the meaning that we bestow upon it. Schutz suggests, in fact, that we cannot truly know anything that transcends our experience of everyday life. The analogies or metaphors that arise from our familiar experience simply help us to "come to terms" with these "unknowable transcendences" (1973, pp. 356, 331). It is hard to see how, on this assumption, political inquiry can yield anything more than a history of political symbols that have proved useful to people at one time or another, depending on their practical purposes.

The manifestationist view that I have sketched is intended to show how political metaphor can be a way after knowledge that leads somewhere, a path that takes us beyond the familiar experience of everyday life to knowledge of political things as they are. It holds that metaphors can take us beyond the observable and also make manifest the intelligible structure of the unobservable. This view presupposes, of course, that political things have an intelligible structure. We have addressed some fundamental questions about knowing the political, but questions obviously remain with regard to the being of the political, or what Schutz refers to as "ontological structure." The ancient question that has been raised anew by Heidegger—What is a thing?—must eventually be raised of the political things.

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The Intelligence of Stupidity: Understanding Failures in Strategic Warning*

STEVE CHAN
Texas A and M University

This analysis discusses the implications of some cognitive and organizational factors for the evaluation and avoidance of failures in strategic warning. It advances three major arguments. First, efforts to assess and improve warning forecasts must take into account the policy context in which they are made and used. They cannot be based on concerns with the accuracy of forecasts alone. Second, important biases are present in retrospective case studies. Therefore, we should accept post hoc explanations of warning failures with appropriate caution. Third, a pluralistic intelligence community, as it is presently proposed for some non-U.S. systems, is unlikely to resolve the problems thought to be responsible for past strategic surprises. It may in fact compound these problems.

In a recent article on earthquake prediction, Richard Kerr (1978) offers an example of how the problems of a weak theoretical understanding and a limited sample of pertinent events can form a vicious cycle that impedes valid forecasts. Inadequate understanding of earthquake mechanisms compels seismologists to pursue an inductive approach, trying to monitor as many likely precursory phenomena as possible. However, attempts to establish the reliability of these indicators by falsifiable prediction are constrained by the lack of a long, detailed monitoring record and by the extreme infrequency of even moderate-sized earthquakes in certain earthquake-prone locales. Moreover, in the absence of a strong theory, researchers cannot explain the inconsistencies in the performance of these indicators and the occurrence of some earthquakes without any apparent warning precursors at all.

These problems of scientific prediction are familiar to social scientists. However, the cognitive and organizational factors involved in issuing and evaluating warning forecasts are less well understood. This article examines some of these factors in the context of strategic warning. It will discuss their implications for explaining and avoiding failures in this type of warning activity (Handel, 1977; Ben-Zvi, 1976; Shlaim, 1976; Whaley, 1973; Hoffmann, 1972; Farago, 1967; Wohlstetter, 1965, 1962; Knorr, 1964; DeWeerd, 1962). This discussion will focus on (1) the difficulties inherent in the nature of strategic warning, (2) the biases that affect post hoc explanations of warning failures, and (3) the limitations of institutional pluralism as a means for ameliorating the causes of these failures.

Nature of Strategic Warning

Strategic warning has the following characteristics:

1. It is concerned with the prediction of a single event (e.g., enemy surprise attack), dichotomizing the probability of its occurrence and the probability of its non-occurrence;
2. The predicted event, if it materializes, will represent a step-level change from the status quo;
3. The occurrence of this event has a significant disutility for the intended recipients of the strategic warning;
4. If they are wise, the intended recipients can act on the basis of the warning to alter the probability of the occurrence of this negative event, or to ameliorate the consequences of this event if it occurs;
5. The chances that corrective actions will succeed are in part dependent on the time lapse between the issuance of the warning and the occurrence of the predicted event; and
6. Whether or not the predicted event will come to pass is ultimately dependent on the actions of the subject of the forecast (i.e., one's adversary).

These characteristics of strategic warning have some important analytical and policy implications. Davis Bobrow (1973a) describes the nature and consequences of Characteristic 1 as follows:

> Warning forecasts attempt to reduce the uncertainty of what is essentially defined as a dichotomous pair of alternatives—the negative event will occur soon or it will not. Given the costs involved, warning forecasts seek to drive

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> the probability of one side of the dichotomy as close to unity as possible. This usually means the negative alternative, since no forecast (special stimulus) is needed for the non-occurrence alternative. The dichotomy burdens the warning forecasts with refuting all alternative explanations of the information which leads to issuing warning. This is particularly true because, given the "bad news" nature of warning, strong motivations are at work to find alternative explanations and thus deny the probable occurrence of the unwanted event. Accordingly, for warning forecasts to be valid they must be durable in a demanding number of ways. First, independent sets of information must converge on the same conclusion. Second, these sets must seem inherently more trustworthy indicators of what is to come than all other sets of available information. Third, the forecasts must have a well-established record for not providing false warning (pp. 17–18).

These considerations are germane to the producers as well as users of strategic warning. Ambiguous signals, "unconfirmed" reports, and the so-called "cry-wolf" syndrome tend to depress confidence in warning forecasts. Thus, failure to issue or accept warning may be based on reasons other than stupidity, treason, wishful thinking, or dereliction of duty. From the viewpoint of the audience of the warning, source credibility is particularly important. It depends in part on the status of the warning purveyor (e.g., the radar operators who detected the approaching Japanese planes to Pearl Harbor), and in part on the perceptions of their motivations. To the extent that the purveyors are perceived to stand to benefit from a change of policy resulting from their warning, their forecasts are suspect in the eyes of the intended receiver. These potential benefits may reflect national interests (e.g., warnings of an impending German invasion of the Soviet Union by Churchill and Roosevelt), bureaucratic missions (e.g., warnings by Ambassadors Grew and Muccio in the Pearl Harbor and Korean War cases), or personal political gains (e.g., Churchill's warning of the danger of appeasement, Senator Keating's warning of Soviet military presence in Cuba). History abounds with warning forecasts based on false expectations or, worse still, contrived for personal or bureaucratic gains. Consequently, to some extent decision makers legitimately discount warnings from dubious sources (e.g., Cuban refugees, spies like Richard Sorge), sources which are ideologically suspicious (e.g., Churchill and Roosevelt for Stalin, Panikkar for Truman), or sources with a track record of false alarms (which happened in all the cases of strategic warning alluded to above).

Another factor that normally decreases confidence in warning forecasts is suggested by Characteristic 2. Strategic warning is concerned with drastic departures from the customary behavioral pattern of one's adversary. Our analytical skills, while useful for assessing incremental changes based on trend extrapolations, are especially weak for anticipating these departures. Also, we are all to varying degrees captives of history. Generals plan for the last war, and diplomats plan for the last peace (e.g., May, 1973; Tuchman, 1972). Certainly, postmortem analyses of strategic surprises provide numerous examples of "conceptual failure" or the inability to grasp how the future may differ radically from the past. Yet it is reasonable to refrain from being overcritical of such individual or collective lack of imagination, for in providing a basis for estimating base frequencies, prior probabilities, and trend directions, history is indispensable for making forecasts.

Characteristics 3 and 4 suggest that the issuance and acceptance of strategic warning are not value-neutral. This self-evident assertion has the less obvious implication that an excessively alarmist posture of "playing it safe" has significant costs. False alarms dull the audience's sense of vigilance, and reduce the credibility and reputation of the individual or organization initiating them. Awareness of these costs can in turn produce the so-called "effectiveness trap," discouraging the issue of uncertain forecasts in the hope of preserving one's effectiveness for a "rainy day."

By their very nature, warning forecasts call for a substantial alteration in the existing patterns of resource and attention commitment, in order to cope with the impending disaster. From the viewpoint of their intended users, acceptance of these forecasts necessarily means to upset to some degree entrenched bureaucratic routines or interests. The resulting reallocation of decision makers' time, resources and attention is likely to be controversial, because these commodities are always in short supply in governments. The political costs can be particularly high when the warning forecast implicitly challenges the basic policy agenda and conception of the regime (e.g., the "Asia First, Europe First" debate in the U.S. government, the "strategic conception" of the Israeli government, the nonalignment policy of the Indian government). Consequently, decision makers have to compare the potential drawbacks to accepting and rejecting a warning. In addition to considerations of bureaucratic consensus and resource scarcity, the disadvantage of acceptance includes the possibility of creating a self-fulfilling prophecy, such as mutual military mobilization leading to a war wanted by neither side. On the other hand, the problem associated with a failure to act on the warning is lessened to the extent that decision makers feel that options are available to dampen the negative effects of inaction (e.g., quick military mobilization, trading space for time)–if and when the predicted negative event comes to pass.

Characteristic 4 has two additional implications. First, strategic warning is contingency forecasting. That is, the occurrence of the predicted event is not inevitable. In fact, the very reason for issuing a warning is to mobilize policy actions to avoid or minimize the chances of an event's occurrence, and failing that, to lessen its impact. Consequently, whether or not the predicted event comes to pass provides a poor basis for evaluating strategic warning. Inaccuracy in the sense of prediction-realization discrepancy alone does not suggest that the warning is invalid, because it may have induced policies that are responsible for averting the undesirable state of affairs predicted (Bobrow, 1973a, pp. 1–2). Conversely, accuracy alone is not enough to prove the validity of the warning. The match between the predicted state of affairs and the eventual historical reality may in fact be a result of self-fulfilling prophecy, haphazard reasoning, or fortuitous occurrence. More importantly, the accuracy criterion provides a poor guide for policy action. As Bobrow has argued, decision makers do not have the luxury to delay actions until all evidence about the accuracy of the warning is available. To hold warning in abeyance in this manner nullifies the very purpose for issuing warning at all.

Second, the above argument suggests that successful warning depends on not only the producer of the warning, but also its intended receiver. If the intended audience does not heed the warning or cannot act on it in pertinent ways, the accuracy of the forecast is immaterial (Belden, 1977; Bobrow, 1973a). Accordingly, attempts to improve the quality of warning forecasts such as those involving the creation of a pluralistic intelligence community cannot assure sound decision making, if they attend only to the incentives and capabilities of the intelligence producers and not the users. We have no reason to expect that decision makers will choose wisely among various competing intelligence estimates and take the necessary actions to cope with the contingency predicted by the chosen estimate.

There is another aspect of the policy context of strategic warning that needs to be taken into account. Even if a warning is accurate, it may be rendered policy irrelevant due to the lack of timeliness (Characteristic 5). One may attempt to lessen this problem by extending the time frame of the warning, increasing the speed of its generation and transmission, developing various contingency plans, or shortening the time required for one's policies to make an impact on the intended target. It is important to recognize that these potential solutions are again not an exclusive function of the content of the warning or the methodology on which it is based. They relate to broader questions about the intended receivers' time horizon, their perception of the costs of vigilance, the resources and attention they allocate for monitoring different issues and actors, and their beliefs about the feasibility and efficacy of alternative policies. These questions are often not treated as part of the strategic warning operation, although they certainly have major effects on the nature and quality of its products. Consequently, we should not hasten to label particular historical episodes as warning successes or failures, without taking into consideration the parameters setting the policy context for intelligence monitoring and forecasts. These parameters may be severely biased against accurate and timely warning, such as the U.S. preoccupation with Europe before the Pearl Harbor attack and the Israeli preoccupation with Arab terrorism before the Yom Kippur War (Handel, 1977; Wohlstetter, 1962).

There is, of course, only so much the defensive party can do to extend the lead time of strategic warning. The offensive party can take the counter-measure of postponing the decision to launch the surprise attack until the last moment. This was apparently what the Japanese, the Germans, and the Arabs did on different occasions (Handel, 1977; Whaley, 1973; Wohlstetter, 1962). The offensive party always has ultimate control over whether the predicted negative event will or will not materialize (Characteristic 6). If it fails to act as predicted, it may be due to the defensive party's increased vigilance, restraining influences from other sources, or simply fortuitous occurrences (e.g., if the Japanese fleet had been sighted on its way to Pearl Harbor). This again suggests that we should not rely exclusively on the accuracy criterion for evaluating strategic warning. While successful warning is by definition self-denying, we do not know whether all non-occurrences of the predicted event are due to successful warning.

The above view also implies that we should refrain from overemphasizing the ubiquity of strategic surprise, since the universe of successful warnings is largely unknown. In this regard, it is also important to differentiate between an enemy's ability to cause surprise through deception and its incentives to do so. In the world we live in and for the foreseeable future, relations between governments are characterized by a significant degree of mutual vulnerability and interdependence. To the extent that one's actions appear surprising to the other party, they are likely to induce various individual- and group-level pathologies mentioned in Charles Hermann's (1969) discussion of the effects of crises. Thus, there are significant incentives to avoid surprising the other party, so that the chances that it will act percipitously are reduced. According to this view, sound crisis management, whether in the case of strategic deterrence or coercive diplomacy (George et al., 1971), requires a reduction of uncertainty for the other side. Collective security pacts, hot lines, and shared warning systems such as the one currently in place in Sinai are just a few of the most obvious examples of attempts to assure clear communication and behavioral predictability (Bobrow, 1975). The strategic surprise felt by some parties to previous conflicts (e.g., the U.S. in the Korean and Quemoy confrontations, the USSR in the Cuban missile crisis, India in the Sino-Indian war) may in fact be more a result of misperception or breakdown in communication than purposeful deception (Whiting, 1975, 1960; Hoffmann, 1972; Knorr, 1964).

Limitations of Retrospective Case Analyses

As in any other type of historical analysis, we engage in retrospective studies of warning failures because we believe that they can yield useful lessons for planning for the future. However, in order to benefit from the conclusions of these studies, we must also be aware of their limitations. As we shall see, they are especially vulnerable to the biases of sampling, availability, anchoring, adjustment, and hindsight. Some aspects of these biases are nonmanipulable from the analyst's perspective because they are inherent in the nature of strategic warning. Other aspects are difficult to correct because human judgment is inherently fallible. Experimental studies have shown that errors caused by some human biases persist even though subjects are warned against them and rewarded for correcting them (e.g., Slovic and Fishhoff, 1977; Tversky and Kahneman, 1975). Therefore, awareness of these biases does not necessarily lead to the alleviation of all their distorting influences. Recognition of the problems they pose, however, should caution us against being overconfident regarding the conclusions of postmortem investigations.

Sampling Bias. Analyses of strategic surprise have to contend with the problem of a small number of pertinent cases. This problem is compounded when we overinterpret the evidence contained in particular case histories. What appear as reasonable cause-effect relationships in a small sample may turn out to be fortuitous correlations. For instance, while the extant literature suggests that cognitive rigidity is a major cause for warning failures, this variable in fact does not discriminate very well between successful and unsuccessful outcomes. Churchill was as much convinced of Hitler's aggressiveness as Chamberlain was convinced of his pacifism. Whatever might be the reasons for the former's "success" and the latter's "failure," cognitive rigidity does not seem to be one of them. Churchill had more "foresight" partly because he was simply more attentive to evidence supporting his preconceptions. Similarly, those who had some anticipation of the other surprise events (e.g., Pearl Harbor, Korea, Cuba), such as Ambassador Grew, Ambassador Muccio, and Senator Keating, did not appear to be more open-minded or less committed to their beliefs about the enemy's motivations than their less clairvoyant colleagues. While history has shown these men to be "right," it is questionable to infer from this that their beliefs were any less rigid or more valid than those of others.

Insensitivity to sample size can lead to unwarranted inferences about population parameters. As suggested earlier, this insensitivity produces the view that strategic surprise is pervasive, when in fact this view is based on a very small portion of the universe of potential instances of surprise. More importantly, it cannot be empirically tested. The null hypothesis (i.e., successes in strategic warning outnumber failures) cannot be tested because instances of success are extremely difficult to ascertain and the universe of these instances is essentially unknowable. Tendencies such as this one lead to unwarranted inferences about the ease of initiating surprise and over-pessimistic expectations about the feasibility of guarding against it.

The above example shows that sampling bias is also related to the failure to take into account base frequencies and the extent to which the selected sample reflects the distribution of these frequencies. Past research on wars and crises has shown that decision makers tend to overestimate rather than underestimate the hostility of their adversaries. Moreover, as

Robert Jervis (1973, p. 134) has commented, "there seem to be more cases of statesmen incorrectly believing others are planning major acts against their interest than of statesmen being lulled by a potential aggressor." Failure to adjust for base frequencies results in "lore-based correlations," such as assigning an unrealistically strong association between failure in strategic warning and outbreak of war. Its unintended effects may include exaggeration of the potential deterrence value of a posture of high vigilance, and exacerbation of the extant tendency of overestimation of the hostility of one's adversary.

Availability Bias. This bias affects sample size and representativeness through accessibility and retrievability. Information constraints compel analysts to focus on cases with relatively easy data access, such as those publicized by open postmortem investigations (e.g., the Stennis Committee, the Agranat Commission). Data access bias usually has the same effect on case selection as retrievability bias, which refers to people's memory of a phenomenon (Bobrow, 1976; Tversky and Kahneman, 1975). Due to familiarity and salience, people more readily recall events that are recent, traumatic, and which happened to oneself or others close to oneself. With regard to the analysis of strategic warning, this bias leads to the over-representation of intelligence failures, threat assessment cases, and one's own experiences or those of one's allies in the recent past. Conversely, they result in the under-representation of intelligence successes, opportunity assessment cases, and the experiences of one's adversaries, particularly if they entail stretching the memory to the distant past. Thus, retrievability bias can distort empirical findings through their influence on sample selection. Note, however, that this bias cannot be attributed to motivational sources such as wishful thinking or self-righteousness, although its effects are no less pernicious or pervasive.

In assessing policy performance, analysts attempt to replicate the problem, event or situation in question by putting themselves in the position of those being evaluated, and ask themselves the question, "Would I have done differently under the circumstances?" This simulation cannot provide a valid (i.e., fair) test due to several biases introduced by availability. The first source of distortion is the availability of outcome. As we will discuss below, experimental results have shown that once people learn of the outcome of an event or research experiment, their images of reality seem irreparably altered (Fishhoff, 1977). Thus, hindsight distorts their retrospective judgments.

The availability bias also influences analyses through the differentiation between relevant and irrelevant information. Many analysts of strategic surprise have commented that the difference between "signal" and "noise" becomes clear only in retrospect. Benefiting from hindsight, post hoc analyses can easily filter out irrelevant information. But the availability bias can distort judgments even in the absence of hindsight. Research on human judgment has shown that subjects given only relevant information make very different estimates as compared to those given both relevant and irrelevant information (Tversky and Kahneman, 1975). The latter consistently made distorted judgments, even though they possessed the necessary evidence for arriving at the correct solutions. It is important to stress that the irrelevant information in this case did not contradict the relevant information; it was merely not pertinent to, and therefore useless for, the judgmental tasks the subjects were asked to perform.

Availability bias can distort post hoc assessment in yet another way through its effects on the amount and timing of information for making judgments. Again, experimental studies on human cognition have shown that the "bulk" of information and the sequence of its presentation can have major effects on perception. In the real world of strategic analysis, warning signals are usually scattered across individuals and bureaucratic units. They are also introduced incrementally over a long period. The consequences are neatly summarized by Jervis (1973):

> Actors can more easily assimilate into their established image of another actor information contradicting that image if the information is transmitted and considered bit by bit than if it comes all at once. In the former case, each piece of discrepant data can be coped with as it arrives and each of the conflicts with the prevailing view will be small enough to go unnoticed, to be dismissed as unimportant, or to necessitate at most a slight modification of the image (e.g., addition of exceptions to the rule). When the information arrives in a block, the contradiction between it and the prevailing view is apt to be much clearer and the probability of major reorganization will be higher (p. 125).

Therefore, people are understandably more often surprised by developments that contradict their images in the real world of strategic warning than in the simulate world of retrospective investigation.

Anchoring and Adjustment Bias. If people come to an estimation problem with divergent initial values (i.e., anchor), they are likely to reach very different conclusions (Tversky and Kahneman, 1975). In other words, people's judgments are affected by the initial definition of a problem, which provides a "launching pad" for subsequent efforts of data search and interpretation (i.e., adjustment). In retrospective analyses, researchers start from a known outcome (e.g., a surprise attack has occurred) and work backwards in time, trying to explain this outcome on the basis of available data. In the real world of strategic analysis, operators start from a dominant conception of the time (which usually assumes that a surprise attack is unlikely) and work forwards, trying to predict alternative courses of development into an unknown future. We have already noted important differences in the available data for these two kinds of analysis. Anchoring bias reminds us that people can be expected to provide different posterior probabilities, simply because their judgments are based on different anterior probabilities. This tendency holds regardless of the distorting influences of availability and hindsight.

Adjustment from anchor is affected by three analytical tendencies. First, while predictive and retrospective analyses start from different initial expectations, they are both biased in favor of hypothesis confirmation rather than hypothesis disconfirmation. To determine whether their views of reality warrant continued confidence, people usually test hypotheses that they expect to be confirmed. This expectation creates cognitive and/or psychological filters that emphasize confirmatory evidence and de-emphasize disconfirming evidence, thus resulting in distorted judgments due to insufficient adjustment from anchor. A corollary of this tendency is that people require weightier evidence before rejecting their expectations than retaining them. This phenomenon may be a result of wishful thinking or a rational decision to hold the apparent disconfirmation in abeyance due to extended prior experience supporting the hypothesis.

Second, as mentioned in the discussion of availability, the sequence of data presentation has an effect on judgments. If the initial feedback confirms a hypothesis, experimental results suggest that people tend to hold on to this hypothesis even though subsequent evidence overwhelmingly denies it (e.g., Matthews and Patton, 1975). As may be inferred from the previous discussion, in retrospective studies the sequence of data presentation is obliterated, if not actually reversed.

Third, forecasts entail chain-like reasoning for estimating the likelihood of different future end states and of alternative paths leading to them. In other words, the analytical task requires serial probability computation based on the estimates assigned to each intermediate event leading to an end state (e.g., surprise attack). While each of these events may have a high probability of occurrence, their conjunctive probability may in fact be quite low. In contrast, in efforts to reconstruct history the method of data appraisal resembles that of multiple regression, with each piece of information being used in an additive manner to account for an outcome (e.g., the occurrence of surprise attack). To push the regression analogy a bit further, convergence between the outcome and antecedent variables is often achieved as a result of considerable multicollinearity among the latter.

Hindsight Bias. Baruch Fishhoff and his associates have carried out a number of experiments to test the effects of hindsight knowledge on human judgments (e.g., Fishhoff, 1977; Slovic and Fishhoff, 1977; Fishhoff and Beyth, 1975). Subjects were asked to estimate the outcome of contrived experiments as well as genuine events (e.g., Nixon's Peking visit). They were then asked to recall their estimates, after having been told the outcome in question. Reports based on hindsight knowledge consistently exaggerated the initial estimates in the direction of the correct answer, thus manifesting the "I-knew-it-all-along" syndrome. This distortion of initial probabilities persisted even when subjects were warned against the possible changes in their judgment induced by the possession of outcome knowledge. Thus, the reported outcomes appeared less surprising to the subjects in hindsight than in foresight.

Slovic and Fishhoff (1977, p. 544) summarize the consequences of hindsight bias as follows: "(a) Reporting the outcome of a historical event increases the perceived likelihood of that outcome and (b) people underestimate the effect of outcome knowledge on their perceptions. As a result, people believe that they would have seen in foresight the relative inevitability of the reported outcome, which in fact was only apparent in hindsight. Thus, they exaggerate the predictability of reported outcomes." With regard to retrospective analyses of warning failures, hindsight bias can also prompt researchers to overemphasize teleological, deterministic interpretations of history, and to ignore the possibility of inadvertent behavior, chance occurrences or other randomness explanations of ostensible "signals" of others' hostile intentions. This bias is, of course, not restricted to ex post facto analyses of historical developments or policy performances. It is also present in the evaluation of scientific research in general, which tends to be assessed in terms of the question, "Did the results surprise me?"

Problems of Pluralism

The failures to anticipate enemy attack by countries such as Israel and India have led some analysts to recommend the creation of a pluralistic intelligence community as a means for avoiding similar surprises in the future (e.g., Shlaim, 1976; Hoffmann, 1972). These recommendations are advanced with the awareness of some limitations of such a system. Thus, one proponent cautions that

> pluralism is not an infallible remedy for the problems that beset the Israeli or any other intelligence system. Pluralism has its own drawbacks and attendant dangers, such as the diversion of resources, the duplication of effort, and the growth of friction and jealousies between the varous branches of the intelligence community. The structural complexity of American wartime intelligence, for example, accentuated the blocks to communication which exist in any large organization, and the intraservice and interservice rivalries that were occasionally carried to the point of withholding secret information. That complexity was among the important causes of the failure to decode properly the mass of signals announcing the impending Japanese attack (Shlaim, 1976, pp. 376–77).

Given this assessment of its potential liabilities, what benefits can we expect to derive from a pluralistic intelligence system? The rationales provided for its adoption usually refer to the consequences of greater institutional autonomy and rivalry. Calls for independent intelligence agencies reflect concerns with the common practices of "back-stopping" and "closed circuit" reporting, as a result of which intelligence products tend to mirror decision makers' opinions (e.g., Shlaim, 1976; McGarvey, 1973; Hilsman, 1956). These practices are degrading as well as dangerous; they create the illusion of confidence on the part of decision makers, and render unlikely the detection of phenomena that are anomalous to their expectations. On the other hand, the argument for institutional rivalry promises to flush out the differences between the products of various intelligence agencies, and thus to force decision makers to attend to the implications of alternative interpretations of the enemy's intentions and capabilities. It is also sometimes argued that the availability of multiple validators enables decision makers to assess the reliability of intelligence appraisals and forecasts. If the analyses of different agencies converge, they inspire more confidence.

These expectations appear rather unrealistic from what we know about one pluralistic intelligence system, that of the United States (e.g., McGarvey, 1973; Blachman, 1973; Blue Ribbon Defense Panel, 1973; Farago, 1967; Wohlstetter, 1962; Hilsman, 1956). Setting aside the important question of whether the time, resources and policy attention needed to sustain a pluralistic intelligence system might be more usefully invested on other purposes, we may question its wisdom for the following reasons.

First, as it is presently proposed (Shalim, 1976), a pluralistic system does not assure a genuine diversity in intellectual perspectives or organizational incentives. It must be emphasized that pluralism has been used to refer predominantly to the institutional representation of various national defense bureaucracies and, to a less extent, the professional status of the operators. It does not refer to the heterogeneity of career aspirations, research methodology or general *weltanschauung*. Instead, we may expect significant homogeneity with regard to all these dimensions. There is little evidence to justify the feeling that civilians are more willing to challenge the "dogma" of the regime than the military people, that experts are more open-minded than non-experts, that the army is less inclined to believe in Soviet belligerency than the air force, that generals are more comfortable with quantitative data and methods than ambassadors, or that foreign and defense ministries have different credibility estimates of alternative information sources (e.g., France versus the Vietminh, the Kuomintang versus the Chinese Communist party, Israel versus the Palestine Liberation Organization). As a result of the very nature of intelligence work, a high premium is placed on the recruitment of those who share the regime's value and belief system. "Deviants" in terms of class background, professional training, ideological commitment (e.g., pacifists), or racial or ethnic origin are systematically under-represented (Wasserman, 1960). Consequently, there is no reason to expect that tendencies of ethnocentrism will be ameliorated or that various cognitive biases will cancel each other out, if we simply increase the number of intelligence bureaus. In fact, the reverse may be true. Errors will be duplicated and distortions compounded as a result of data redundancy, systemic biases and the dynamics of "groupthink" (Janis

1972), leading to an illusory confidence in the intelligence products.

Second, even assuming that it is desirable to have independent intelligence bureaus, it may not be feasible to achieve this objective. A seller's autonomy depends in part on the number of competitive buyers (Bobrow, 1973b). We all know, however, that the market for intelligence products is dominated by a single buyer (i.e., the national defense community) and, unlike some other policy products, there are few alternative consumers of intelligence (e.g., Congress, public interest groups). Proliferating the number of sellers without expanding the number or competitiveness of the buyers results in a decrease, and not increase, in the suppliers' independence.

Third, the above argument also indicates that the objectives of institutional autonomy and rivalry may be incompatible. Presumably, a truly independent organization is one with a monopolistic control over the supply of certain goods or services, for which there is an inelastic demand and little prospect of substitution. By increasing the number of intelligence suppliers without changing the buyer's incentives, we indeed encourage competition among them, but only of the sort intended to ingratiate the buyer. Tendencies toward "back-stopping" and "closed circuit" reporting will be exacerbated under these conditions. Moreover, conformity to organizational norms will be emphasized. "Devil's advocates" within each of the competing bureaus will be silenced, and dissenters and doubters "domesticated" (Thomson, 1973). Finally, intelligence monitoring and forecasting efforts will increasingly reflect organizational missions rather than policy problems.

Fourth, even accepting the view that there are important differences in interests and perspectives among members of the national defense community, it does not necessarily follow that a pluralistic intelligence system will articulate rather than conceal interagency differences. The U.S. experience has shown that the information forwarded to decision makers is often the product of bureaucratic compromise to "plaster over" or "bargain out" these differences (e.g., McGarvey, 1973; Blue Ribbon Defense Panel, 1973). And, on the relatively rare occasions that interagency differences are publicized, they are as likely to be based on concerns with organizational missions as analytical considerations. Moreover, differences in analytical considerations tend to be restricted to issues of technical sufficiency and cost-benefit ratios. Like other bureaucrats, intelligence analysts have to conform to the regime's basic views about the nature and morality of international relations if they wish to be treated as "responsible" and "serious." Therefore, they refrain from asking the really "tough" but crucial questions such as the aggressiveness of the Soviet Union, the morality of the Vietnam War, and the validity of the "domino theory."

Fifth, even assuming that decision makers are presented with meaningful alternatives of intelligence appraisal, there is no reason to expect that they will choose wisely. Their choice tends to reflect personal experience and operational responsibility, considerations of institutional loyalty and source credibility, and general familiarity with the content and methodology of particular intelligence estimates. Leisurely examination of alternatives, especially during crises, is often impossible. Nor should we assume that they have the necessary qualifications to exercise the judgments needed, since they tend to be generalists rather than specialists in their training. Finally, even assuming that the validity of a warning is not in doubt (e.g., the current energy crisis, the news of the Pearl Harbor attack reaching General MacArthur in the Philippines), the relevant decision makers may not have the power, authority or wisdom to cope with the impending disaster.

Finally, it should be stressed that an intelligence system totally divorced from the thinking of the political leadership is neither practical nor desirable. Intelligence operations inevitably reflect to some extent the decision makers' prior commitments to particular beliefs, values and resource investment. More importantly, in the absence of a referent system provided by these commitments, efforts to collect and analyze information will "drift" aimlessly. These efforts may be based on objectives, assumptions and methods that vary substantially across different intelligence producers. Their results are likely to reflect the operational concerns and parochial interests of the producers and, to the extent that the rationales underlying them are not explicated, decision makers lack a rational basis for comparing the different estimates and recommendations offered to them. Alternatively, if decision makers find these rationales unconvincing or unfamiliar, they are again unlikely to act on the warning forecasts, thus nullifying the very reason for having these forecasts at all.

Since there is no such thing as a "policy-neutral" intelligence estimate or forecast, the question is whose operational values, assumptions and rules of interpretation should provide the frame of analysis. In practical terms, an intelligence system divorced from the thinking of the political leadership results in substituting the rationales of producing analysts or agencies for those of decision makers. As mentioned above, this is unsatisfactory because decision makers may be unable to control for or agree with the different rationales of these sources. On the other hand, an explicit policy "conception" of the leaders can be used to guide the search and processing of information. Without the basis provided by such a "conception," it is difficult to engage in reasoned criticism and improvement of policies, and to assess the performance of decision makers and intelligence operators alike (Meehan, 1971). This view does not imply that intelligence operators should merely mirror the leaders' decision logics. It suggests that the leaders' decision logics should be used as an "anchor" for conducting intelligence analysis and "multiple advocacy" (George, 1972; Destler, 1972). Thus, it does not preclude, for example, the establishment of an agency serving as a depository of evidence challenging these logics, such as the one proposed by Joseph de Rivera (1968). The incentives for the effective performance of such an agency, however, are neither evident in the current U.S. system, nor provided for in calls for a pluralistic intelligence community.

Conclusions

We have examined some cognitive and organizational factors related to the evaluation and management of strategic warning, and have advanced three major arguments. First, the assessment of warning forecasts has to take into account the policy purpose and context of these forecasts. Efforts to improve the quality and pertinence of these forecasts cannot focus solely on the capabilities and incentives of their producers, but must also attend to those of their intended receivers. This conclusion also suggests that the accuracy of forecasts is in itself inadequate for understanding or avoiding failures in strategic warning. It is a necessary, but not sufficient, condition for successful warning. Yet it cannot be used as a basis for decision making, since knowledge about possible prediction-realization discrepancy is necessarily unavailable at the time of decision making and can only be inferred retrospectively.

Second, post hoc case studies of warning failures are vulnerable to several important cognitive biases. Accordingly, we should be very cautious in accepting their conclusions and in generalizing them across problems and situations. While it is difficult to control the distorting influences of some of these biases, an explicit recognition of them can reduce the danger of overconfidence in the validity of post hoc explanations.

Third, a proliferation of intelligence bureaus alone is unlikely to increase their institutional autonomy or diversity. And, even assuming that the achievement of these objectives is feasible, it is still debatable whether this will be desirable in view of its negative implications for policy control and political accountability. Thus, the promised benefits of a pluralistic intelligence system may turn out to be much more elusive than its costs.

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COMMUNICATIONS

Inflation, Unemployment, and Left-Wing Political Parties: A Reanalysis

TO THE EDITOR:

The errors and omissions in "Political Parties and Macroeconomic Policy" by Douglas A. Hibbs, Jr. (*American Political Science Review*, December 1977) are so serious that they ought to be called to the attention of *APSR* readers. Hibbs asserts the existence of several basic relationships between economic and political variables which, if true, would have important policy implications. No doubt many readers will have spotted the flaws in Hibbs' analysis on their own, but others may well be taken in. Hence the necessity for this consumer product safety report.

Hibbs advances and purports to test three hypotheses:

1. On a cross-national basis, the level of inflation is inversely associated with the level of unemployment;
2. On a cross-national basis, the level of left-wing government is positively associated with the level of inflation;
3. On a cross-national basis, the level of left-wing government is inversely associated with the level of unemployment.

An attempt to replicate Hibbs' tests, especially of hypotheses 2 and 3, reveals that he has committed a number of errors, namely:

1. biased selection of cases;
2. combining noncomparable data sets;
3. biased selection of the time period for the independent variables;
4. subjective (unreliable) definition of one part of the independent variable;
5. an invalid definition for the other part of the independent variable.

I treat the above points in order.

1. Cross-national unemployment data lack comparability. For this reason, apparently, Hibbs relies on an article in the *Monthly Labor Review* which made an effort to present comparable unemployment data for the U.S. and seven foreign countries. It would therefore be reasonable for him to have applied his tests to these eight countries for which his source supplied comparable unemployment data. Hibbs does not do this. Instead, he assembles his cases on what appears to be a subjective basis. The *Monthly Labor Review* article gives data for eight countries. Without a word, Hibbs discards Japan from this data set. As it happens, Japan dramatically contradicts hypothesis 3: it had extremely low unemployment and no left-wing government. Surely it is wrong to discard a hypothesis-contradicting case from a data set without advising the reader.

Next, Hibbs adds five selected countries to the seven remaining in the *Monthly Labor Review* group. The data for these countries come from ILO-OECD sources (see below). Hibbs does not include all countries given by these sources, nor even all democracies. He discards all but five: Denmark, Finland, Norway, Netherlands, and Belgium. His ILO-OECD sources, however, give data for many other perfectly respectable democracies, including Australia, Austria, Iceland, Ireland, Luxembourg, New Zealand, and Switzerland. These countries are never mentioned.

As one inspects these omitted cases, two points emerge. First, once we leave the *Monthly Labor Review* data set we encounter a swamp of noncomparable unemployment data. As I discuss below, the ILO-OECD data are unusable for cross-national analysis; Hibbs should not have depended upon them for *any* cases.

Secondly, if one does depend upon these data—as Hibbs does—it emerges that some of the cases Hibbs is discarding squarely contradict his arguments. Hypothesis 1, for example, is seriously challenged by the cases of Ireland (high inflation, high unemployment) and Australia (low inflation, low unemployment). Switzerland, with very low unemployment and little left-wing government, contradicts hypothesis 3.

The point is obvious: you should not discard cases from data sets without (a) saying you are doing so, and (b) cogently defending your decision. In an extreme case, violation of this principle will enable a researcher to claim support for any hypothesis simply by ignoring contradictory cases.

2. Using Hibbs' stated source, the ILO *Yearbooks*, I was unable to arrive at the same figures he presents. The discrepancy was especially sharp in the case of Denmark's unemployment figure: Hibbs reported 1.5 percent; I got 3.2 percent. I wrote him about this and received the reply that his footnote is incorrect. Instead of the ILO source which he said he used, he used an OECD publication, not cited, as the source for the unemployment data for the five additional countries. The following

point applies to both of these sources (ILO-OECD).

Cross-national unemployment data are notoriously noncomparable. Countries use different definitions: different age groups are involved, different questions are asked in surveys—if surveys are used—and so on. The *Monthly Labor Review* article represents an effort by a team of labor statisticians to present some meaningful cross-national data. The ILO-OECD sources contain figures which are not comparable with these.

For example, for the United Kingdom the unemployment rate calculated from OECD figures (according to the instructions Hibbs gave in his letter) works out to be 1.6 percent for 1960–69. The *Monthly Labor Review* gives 2.9 percent for the same period. Obviously the two sources are not supplying comparable data. Numbers called the same thing (e.g., "unemployment") do not necessarily mean the same thing. Yet Hibbs uses the *Monthly Labor Review* figures for seven countries and the OECD data for five others.

Hibbs may believe that the OECD-ILO data are comparable for the five countries he includes. But with a question as complex as the cross-national comparability of unemployment figures, if one advocates an hypothesis, one should consider oneself a biased judge, likely to allow the needs of his hypothesis to influence one's opinion of which cases are acceptable. The use of a poor data set in its entirety has at least the defense of objectivity; the use of subjectively selected cases from such a set is more difficult to countenance.

3. Hibbs chooses the period 1960–69 to measure unemployment and inflation. Once this period is chosen for unemployment and inflation data, what would be the reasonable period to use to measure the independent variable, left-wing rule? One answer would be 1960–69, the same period. Another possibility would be a slightly earlier period such as 1958–67 to allow for a lag between policies and their presumed effects. Hibbs eschews these obvious possibilities and adopts instead the period 1945–69. The effect of this decision is to increase the left-wing government scores of several of the European countries and thus bring them closer to the desired regression line. France, for example, has had no left-wing government (by Hibbs' own declaration) since 1958. A zero left-wing score for France seriously disrupts the scattergrams. By counting Socialist presence in French governments before 1958, the French left-wing score becomes 35. The left-wing score of Norway is similarly increased by counting earlier periods of Labor party rule.

Hibbs explains this curious choice of time period in footnote 16, asking us to believe that long lags, of several decades, between party rule and macroeconomic result are implied in his formulation of hypotheses 2 and 3. While it is perhaps implausible, we might accept this contention if the author consistently adhered to it. But he does not. The latter section of the article is devoted to a time series analysis which purports to show that after the British Labour and U.S. Democratic parties take over, unemployment rates in their respective countries go down. In order to undertake that analysis at all, one has to assume that macroeconomic effects come relatively quickly. And Hibbs explicitly makes this assumption, postulating a three-month lag between change in party control and unemployment effects. The results themselves—and Hibbs, at least, is obligated to believe them—indicate quick response times. For the British case, for example, Hibbs claims that the effect of party control on unemployment is virtually exhausted after about six quarters, that is, a year and a half, and completely exhausted in four years. It is curious indeed to see an author repudiate in one place his own findings in another.

Consistency, in fact, is not one of Hibbs' strengths. In footnote 15, just before the footnote 16 which argues for the "decades lag" interpretation of the hypotheses, Hibbs writes:

> Actually there was one brief period of Socialist-led rule in France after 1951: Guy Mollet's government of February 1956 to May 1957. Analysis of annual data shows that unemployment was lower and inflation higher *during* [italics added] Mollet's government (as well as *during* [italics added] the subsequent Center-Left government of Bourgès-Maunoury) than during the right-wing Gaullist governments of the late 1950s and 1960s.

Clearly, if Hibbs believes in the "decades lag" argument of footnote 16 he should not in footnote 15 be giving Mollet credit for an unemployment level during his own regime. Obviously Hibbs is playing both sides of the street. De Gaulle is blamed for a 1960s unemployment level which is relatively higher in one comparison; then his Socialist predecessors are given credit for exactly the same unemployment level which is now relatively lower in another comparison (with the U.S. and Canada).

4. Hibbs does not offer an explicit operational definition for a "left-wing" political party. In practice, it appears that he depends on a semantic definition, considering the labels "Socialist," "Labor," and "Social Democrat" as

being "left-wing." The policy orientations and decisions of these parties go unexamined. Thus the moderate, centrist Norwegian Labor party and Danish Social Democrats get classified as "left-wing." The American Democratic party is right-wing.

Here again Hibbs falls into inconsistency, for he contradicts his treatment in the latter part of the article. There he treats the Democrats, with "relatively close connections to organized labor and lower income and occupational status groups" as the left-wing party which allegedly lowers unemployment by 2.36 percent when it gains control of the executive.

The inconsistency can hardly be attributed to random carelessness. In the analysis concerning the cross-national hypotheses 2 and 3, it is necessary that the U.S. be zero percent "left-wing" governed if the correlations are to come out as hypothesized.

As Hibbs employs it, then, the left-wing, right-wing dimension is a rubber variable, a variable so poorly defined that the same case (i.e., the U.S. Democratic party) may be placed in opposite categories at the discretion of the researcher.

5. In his hypotheses and the arguments supporting them, Hibbs refers to the general notion of left-wing *government,* that is, left-wing control of governmental decisions. It is his argument that left-wingers, to the extent that their preferences prevail, opt for higher inflation and lower unemployment. Clearly, then, to test his hypotheses we need a measure of left-wing government which fairly reflects the degree of control of these designated parties over governmental decisions. Hibbs' approach does not serve this purpose. First, he considers only the executive branch of government, and secondly, he treats any left-wing party presence in a parliamentary coalition executive as amounting to total left-wing control of government in that year. The result is a definition which is an invalid measure of the meaning of the term "left-wing government."

For example, in 1962 and 1963 the tiny Italian Social Democratic party, with 4 percent of the seats in the lower house, was one of the minor members of the Christian Democratic party governing coalition. According to Hibbs' practice, Italy is scored as having a "left-wing government" in those years. Similarly, Sweden is calculated by Hibbs to be 100 percent left-wing throughout 1945–69, even though it is widely known that a pattern of "consensus government" prevailed there in which all parties participated in compromise decisions. While the Social Democrats have formed the Cabinet, they typically have done so with a minority of the seats in the Riksdag and at the suffernce of the other parties.

What is the effect of adopting this curiously invalid measure of left-wing government? It exaggerates the left-wing scores of many European countries. The basic requirement for fitting the data to hypotheses 2 and 3 is to separate the U.S. and Canada from the European countries Hibbs has selected. In this period the U.S. and Canada had higher unemployment and lower inflation than the European countries. The U.S. and Canada, assigned zero left scores, anchor one end of the scattergram. Any technique which increases the left scores of the European countries will therefore produce some nifty-looking scattergrams—Japan having been excluded at the outset. Hibbs' method of exaggerating left-wing government scores of European coalition governments, then, assists in making the data conform to his hypotheses.

The extent to which these systematic abuses aid Hibbs' case is perhaps best illustrated by the results obtained in a replication free of these abuses. For this replication the following decision principles were observed:

1. The countries employed are the eight originally encountered by Hibbs in the *Monthly Labor Review* article for which meaningful cross-national unemployment data exist. The unemployment data for other countries are so manifestly noncomparable that it is difficult to expand the analysis to them.
2. The period for the inflation and unemployment variables is 1960–69, the period chosen by Hibbs.
3. The period for the party rule variable is also 1960–69, it being apparent that Hibbs, except for one lapse, believes short or negligible lag-times exist between party control and economic result.
4. Left-wing parties are, following Hibbs' semantic approach, any party called "Socialist," "Labor," or "Social Democrat." To the list of left parties we must add the U.S. Democratic party, following Hibbs' own treatment in the second part of the article. Canada, again yielding to Hibbs, is considered 100 percent "right-wing," Mr. Trudeau notwithstanding.
5. Party control is measured, again following Hibbs, only for the executive branch. For coalition governments in parliamentary regimes, a left-wing party is assigned the fraction of control according to the proportion of seats it holds in the lower house. The debatable Swedish case is resolved in Hibbs' favor, being

considered 100 percent left-wing throughout 1960–69.

The data which result from the application of these decisions are shown in Table 1. The product-moment correlation between the inflation and unemployment figures (Hibbs reports –.45, N=12) is r = –.49, which is not statistically significant at the .1 level (one-tail; t=–1.37, df=6).

The two political hypotheses (2 and 3) are unsupported by the data. The correlation between the degree of left-wing government and inflation is r = –.21 (Hibbs reports +.74). The correlation between the degree of left-wing government and unemployment is r = +.09 (Hibbs reports –.68).

Naturally, other data sets could have been employed and other definitions of the terms adopted. I have explored as many of these other possibilities as I could think of, and I see no sign with any of them that the data, handled fairly, will support the hypotheses Hibbs has advanced.

The last section of Hibbs' article contains a multiple regression time series analysis which purports to show that the British Labour and American Democratic parties reduce unemployment in their respective countries when they gain control of the executive. Here again we encounter the same pattern of abuses and inconsistencies.

Consider Hibbs' treatment of the British experience. The yearly unemployment figures for 1948–1976 are given in Figure 1. Do these data support the contention that Labour is the party of lower unemployment? We notice, first, that unemployment shoots up to history-making levels after Labour comes to office in March 1974. Second, we notice a substantial increase in unemployment during Labour's earlier rule of October 1964-June 1970. Again, unemployment reaches levels not previously attained in the period. (The lowest unemployment level, in 1955, occurs under the Conservatives.) Finally, we notice a sharp increase in unemployment in the first part of the Conservative regime beginning June 1970, but the level falls back in 1973, leaving the case a draw. Surely, in the face of these data, an advocate of the proposition that the Labour party reduces unemployment would be confounded. But Hibbs is scarcely slowed down.

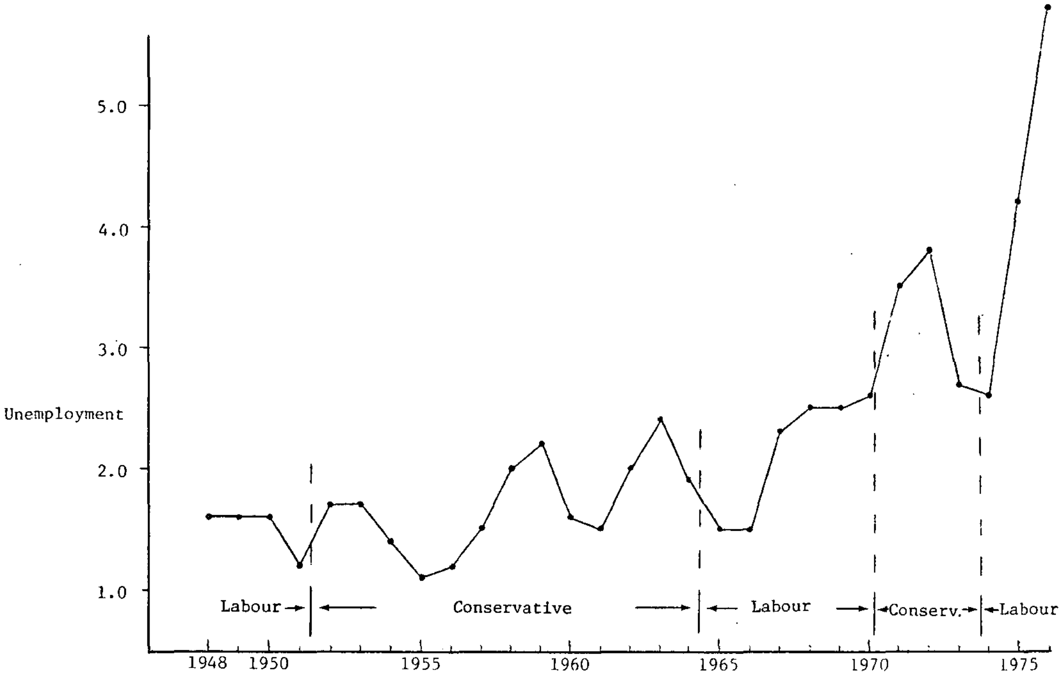
First, the figures of 1973 onward are discarded "intentionally," as he says in footnote 27, on the grounds of an "exogenously imposed economic crisis." Strangely, Hibbs fails to mention what this crisis is. The only one I can think of is the oil embargo and the doubling of oil prices by OPEC. But these took place at the *end* of 1973 and therefore should not be the basis for discarding 1973 data. Blotting out the 1973 data is, however, helpful to Hibbs' case: the Conservatives get the blame for the sharp 1970–72 increase in unemployment, but they are denied credit for the equally sharp decline in 1972–73.

To handle the sharp rise in unemployment under Labour in 1967–69, Hibbs adds a dummy variable to the equation taking a value of 0 prior to 1967 and 1 otherwise. The main effect of this decision is to let the dummy, not the Labour party, take the blame for the higher 1967–69 unemployment.

Hibbs' explanation for introducing the dummy variable is that in October 1966 the Labour party introduced an expanded unemployment compensation system. Hibbs argues that "the new compensation scheme increased the rate and duration of unemployment." Another contradiction. Hibbs starts out asserting and ends up claiming that the Labour party reduces unemployment. But now, to put inconvenient data in their place, he asserts the opposite: the Labour party increased unemployment. Thus Hibbs presents us with a beast which I thought, until now, was purely mythological: the rubber

Table 1.

Country	Percent Left-Wing Executive 1960–69	Average Unemployment 1960–69	Average Rate of Inflation 1960–69
United States	80	4.8	2.3
Canada	0	5.1	2.5
United Kingdom	50	2.9	3.5
Japan	0	1.3	5.0
West Germany	12	0.6	2.5
Italy	12	3.7	3.6
Sweden	100	1.7	3.7
France	0	2.3	3.8



Source: ILO, *Yearbook of Labor Statistics*

Figure 1. Unemployment in the United Kingdom, 1948–1976

hypothesis, true or false depending on the researcher's needs at the moment.

In conclusion, Hibbs has failed to support his hypotheses about inflation, unemployment, and left-wing parties. He has, however, resoundly demonstrated the old saw, "You can prove anything with statistics." The proper statement of this adage is, of course, "You can prove anything with the abuse of statistics."

JAMES L. PAYNE

Texas A&M University

TO THE EDITOR:

James Payne's communication (1979) concerning my article "Political Parties and Macroeconomic Policy" (1977) gives the impression that he believes he is onto something really big. (Perhaps I should be flattered?) I will try to salvage my reputation by convincing the *Review*'s readers that the "errors and omissions" claimed in his "consumer product safety report" amount to pretty small potatoes.

Readers may recall that my principal argument was that governments pursue macroeconomic policies broadly consistent with the objective economic interests and subjective preferences[1] of their class-defined core political constituencies. Labor-oriented, working class-based leftist parties typically attach greater weight to full employment than price stability, whereas the reverse is generally true for middle- and upper middle class-based centrist and rightist parties. Most of the article was devoted to showing that these partisan differences in economic priorities have an impact on observable macroeconomic outcomes, particularly unemployment outcomes. Let me now respond to Payne's charges.

Selection of Cases. Payne implies that I selected cases (countries) that supported my arguments and excluded those that did not—a rather serious accusation. Actually, the countries appearing in my article form the empirical base of a much larger project on the political economy of Western industrial democracies, and have been used in several subsequent papers dealing with such diverse political-economic phenomena as trade union militancy and wage inflation, long-run trends in strike activity, and the response of monetary authorities to the OPEC supply shock. The countries were selected initially because of their obvious importance in the political economy of Western capitalism (the United States, West Germany, etc.) and/or because they figure prominently in the literature on important political phenomena of interest to me (e.g., the Scandinavian welfare states). For the purposes of my work on macro political economy in Western industrial societies, the countries identified in ILO-OECD publications by Payne—Australia, Austria, Iceland, Ireland, Luxembourg, New Zealand and Switzerland—satisfied neither criterion.

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[1] My article summarized the objective distributional consequences of and subjective preferences toward unemployment/inflation configurations. These topics are given more sustained analysis in Hibbs (1975, 1978a, 1978b).

Non-comparable Data. I have corresponded with Payne about Danish unemployment, pointing out that Danish specialists agree that trade union figures provide an accurate count of unemployment in the private sector, that there is virtually no unemployment outside the private sector, and, therefore, that the trade union figures in conjunction with data on total labor force size yield reliable economy-wide unemployment rates (an average of about 1.5 percent for 1960–69). I stand on this statement.

Payne's more general point, that there are serious problems associated with making international comparisons of unemployment, specifically when the data are based on registration statistics rather than labor force surveys, is well taken. However, as I will show ahead, if international comparisons are undertaken for *all* industrial democracies for which comparable data are available, my principal argument is supported.

Selection of Time Periods. Payne noticed that in my international comparisons I looked at average rates of unemployment and inflation during the 1960–69 period in relation to "left-wing rule" over the period 1945–69, whereas in my dynamic time-series analyses of unemployment outcomes in the United States and Great Britain the partisan political terms entered the intervention models with comparatively short lags. He concludes that this is inconsistent and contradictory, and is a major illustration of the systematic abuses in my article.

I have a higher opinion of my colleagues' critical judgment than this (not so) sleight of hand implies. Payne's remarks here apparently stem from his failure to grasp the distinction between long-run equilibria and shorter-run dynamic fluctuations and/or to understand my ideas about the political sources of economic outcomes, which are quite clearly stated in my article (pp. 1473–75). The political factors bearing on macroeconomic outcomes that I identified included:

(1) partisan differences in the political authorities' short-run manipulation of monetary and fiscal policy instruments;

(2) the postwar role of the Left in shaping institutional arrangements (principally labor market and manpower policies) designed to minimize unemployment, which are very difficult to dismantle once they are in place;

(3) the Left's prior economic performance, particularly just after the Second World War when many feared the onset of another great depression, and continued emphasis on low unemployment in political discourse which generated widespread public expectations of sustained high employment and therefore constrained the behavior of all subsequent governments.

Since I argued that factors (2) and (3) make an important contribution to explaining cross-national variation in aggregate, equilibrium macroeconomic outcomes, I computed the "left-wing" government scores over the 1945–69 period in my international comparisons. Payne points out that this enhances the associations in the scattergrams, which of course is precisely what my reasoning predicts.

By the early to mid-1950s the structural-institutional factors just mentioned were largely fixed in most capitalist industrial democracies. The stochastic time-series analyses in the latter part of my article were designed to estimate movements in unemployment rates that plausibly could be attributed to interparty differences in monetary and fiscal policies, which indeed have relatively short lag times.

While I thought this distinction between long-run equilibria and short-term fluctuations was obvious in my article, apparently it was not. Let me try to illustrate the point. Suppose we observed a group of countries experiencing partisan variation in control of the executive and that the outcome variables were mean stationary. If we fit time-series intervention models to the outcome variables for each nation, the party intervention terms would estimate the impact of partisan changes on fluctuations in the outcome variables and the intercept constants would pick up the net effects of nation-specific, (relatively) time-invariant structural-institutional factors not captured by the ARIMA functional forms.[2] Cross-national analysis of the intercept constants or location parameters would be akin to the international comparisons of equilibrium outcomes described earlier.[3]

Definition of the Independent Variable. Let me identify what I believe are errors of fact and judgment among Payne's observations. Payne asserts that I scored Italy as having a "left-wing" government in 1962 and 1963 because the tiny Italian Social Democratic party was a minor member of the Christian Democratic governing coalition. I did not. The PSDI is socialist or social democratic in name only and, notwithstanding Payne's remark about my use of a semantic political classification scheme, it made no contribution to my computations.

Payne describes the Norwegian Labor party and the Danish Social Democrats as moderate and centrist and seems to find it odd that I consider the American Democratic party more rightist by comparison. Further on Payne characterizes the postwar rule of the Swedish Social Democrats as "consensus government . . . in which all parties participated in compromise decisions." These observations will no doubt be the source of some amusement to the *Review*'s Scandinavian readership.

From the time the Labor or Social Democratic parties in the Scandinavian welfare states achieved political power in the 1930s or just after the Second World War until the culmination of the development of the system of collective consumption and public sector distribution of income in the early 1970s, the growth in the share of the national product allocated by the state was 24, 28 and 30 percent in Denmark, Norway and Sweden, respectively, as compared to a public sector increase of just over 5 percent in the United States.[4] By 1972 the fraction of national income (again exclusive of defense and nationalized industries) ultimately distributed by the state in these Scandinavian countries ranged between 45 and 50 percent, but stood only at about 25 percent in the United States. Average and marginal rates of taxation tell a similar story. By socializing the consumption and final allocation (though typically not the production) of an enormous fraction of the national product, the Scandinavian Social Democrats engineered a massive circumvention of the private market, which fundamentally changed the political economy of distribution. The American Democratic Party has never entertained a program as ambitious and the private marketplace continues to dominate distributional outcomes in this country.

In *international* perspective, then, I view the Democratic party as centrist (not rightist, as Payne says). However, for purposes of *intranational* political analysis the Democrats clearly should be treated as the party of the Left, though, as I observe in my article, the two dominant American parties are less distant ideologically and have more heterogeneous social bases than the British Labour and Conservative parties or, for that matter, the major political blocs in most advanced capitalist democracies. I doubt many political scientists would find this international/intranational distinction unreasonable or difficult to grasp.

In any case, the abstract point is of little moment. If my assumption that the Democrats (as the party with the closer connections to organized labor and lower status groups) generally assign higher priority to unemployment than the Republicans was incorrect, my intranational time-series analysis for the U.S.—which essentially amounted to a dynamic linear contrast of interparty unemployment performance—would not have produced the results reported in my article.

The impression left by Payne's remarks about the consensual nature of postwar Swedish political life is quite misleading and reflects a touching political innocence. The basic postwar strategy of the Social Democrats has been to divide the bourgeois opposition—isolating the Conservatives (now called the Moderate party) on the right wing of the Swedish spectrum—by selectively forming coalitions and making compromises on discrete political issues. Throughout the period they have implicitly relied on the Swedish Communists' refusal to be implicated in turning a Social Democratic government out of office. It is important to understand, moreover, that the terms of "consensus" policies were for the most part imposed rather than negotiated by the Social Democrats and their extremely powerful trade union allies. After all, the Social Democrats presided over the establishment of what is probably the most advanced welfare state in the West and there have been many socioeconomic losers during the process. Nonetheless, outbreaks of bitter confrontation politics over such issues as the state's role in economic coordination and planning, steep rates of taxation, and the national compulsory pension scheme have periodically rocked the political system. A major struggle over the LO/Meidner plan to transfer ownership and control of large privately held firms to the trade unions looms on the horizon.

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[2] For another illustration in the context of a different research problem, see Hibbs (1976), especially pp. 1046–47.

[3] Strictly speaking, for this analogy to hold, the various country models should have similar stochastic structures and regular oscillation in partisan control of the government. Nonetheless, the example illustrates the point.

[4] Exclusive of defense and the budgets of nationalized enterprises. For further discussion and sources for the figures presented here and below see Hibbs (1978c).

Replication of the International Empirical Results. In his replication of my international comparisons, Payne computes leftist executive influence over the period 1960–69 and, more important, scores the United States as much closer to Sweden than, for example, to Canada. I do not think this makes much sense and therefore see no point in commenting on his results. However, I do accept in principle Payne's criticism of my computation of leftist executive participation scores (percentage of years during which leftist parties occupied cabinet posts). In subsequent work I have used more reasonable measures–the average fraction of cabinet posts held by the Left, and the Left's share of the popular vote–to measure executive participation and electoral political importance, respectively. Cabinet share scores reflect the Left's direct participation in executive policy making. Vote share scores are an indirect measure of the Left's political influence, since all governments are likely to be constrained significantly on issues of great importance (e.g., employment) to a trade union-based, socialist-labor political bloc supported by a large fraction of the electorate.

Table 1 reports the simple correlations of the two Left influence measures just described as well as the executive participation indicator used in my original article with the rates of unemployment and inflation in 11 industrial democracies. In response to Payne's legitimate complaint about the international comparability of unemployment rates for some of the countries appearing in my article, I used only nations for which (1) unemployment data adjusted by the U.S. Bureau of Labor Statistics were available and/or (2) unemployment was determined by labor force sample surveys, to compute the correlations.[5] Since many of the underling scatterplots exhibit nonlinearities, I report linear and semi-log linear correlations.

The results in Table 1 show the following:

Table 1. Linear and Semi-Log Linear Correlations: Unemployment, Inflation and Political Importance of the Left in 11 Industrial Democracies[a]

	Average Unemployment Rate 1960–69	Average Inflation Rate 1960–69
Average Left Vote Share 1950–69	–.70	.48
log(*x*+1)	–.66	.46
Average Left Vote Share 1960–69	–.70	.48
log(*x*+1)	–.66	.46
Average Left Cabinet Share 1945–69	–.38	.36
log(*x*+1)	–.53	.28
Average Left Cabinet Share 1960–69	–.27	.31
log(*x*+1)	–.33	.35
Average Percent Years Left in Cabinet 1945–69	–.41	.50
log(*x*+1)	–.52	.30
Average Percent Years Left in Cabinet 1960–69	–.30	.32
log(*x*+1)	–.34	.32

Source: Computed from a data appendix prepared by Nicholas Vasilatos. Supplied upon request to the author.

[a] Australia, Canada, Denmark, Finland, France, Italy, Japan, Sweden, U.K., U.S., W. Germany.

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[5] BLS officials do adjust the data of countries that rely on labor force surveys to ascertain unemployment, but averaged over several years the adjustment factors are negligible to vanishing. To illustrate, the (upward) 1960–69 average adjustment proportions for the countries using labor force surveys that have been studied by BLS statisticians are: Australia, 0.0; Canada, 0.0; Italy, 0.09; Japan, 0.02; Sweden, 0.0.

Denmark also appears among the countries listed in Table 1 for the reasons mentioned previously.

(1) My basic argument is supported by every correlation. The political indicators correlate negatively with the average unemployment rate and positively with the average inflation rate.

(2) Every correlation but one is consistent with my a priori reasoning concerning the time-wise conceptualization of the political indicators. The 1945–69 (1950–69) indicators exhibit stronger associations with the macroeconomic variables than the 1960–69 indicators. The vote share variables show virtually identical correlations across time periods because electoral support for the Left is extremely stable through time (correlating, for example, at 0.997 across the two periods in Table 1).

(3) A high Left vote share, even if it does not lead to frequent outright control of the executive, may exert substantial influence on macroeconomic policies and outcomes, at least with respect to issues of great salience to the organized working class.

Of course, the correlations in Table 1 as well as the international comparisons in my original article *prove* nothing. The underlying model and methodology are far too simple to support strong claims. The results of the international comparisons are merely consistent with the basic arguments I have made concerning the political economy of macroeconomic policy in industrial democracies.

Remarks About the Intranational Time-Series Analyses. In this section Payne again accuses me of trying to buffalo the *Review*'s readers by

(1) adding a dummy variable to the British model, which had a value 0 prior to 1966:4 and 1 otherwise, so that the Labour party would not "take the blame for the higher 1967–69 unemployment," and (2) not carrying my time-series analyses through to 1973, a year during which the Conservatives were in office and unemployment fell in Great Britain.

Let me first clear up Payne's misunderstanding of the contribution made by the dummy variable (C_t in my article) to the interparty unemployment performance estimate for Great Britain. The steady state effect on unemployment attributed to this variable (0.86 percent) in the U.K. model affects *all* subsequent unemployment observations equally. Since the numbers of Labor- and Conservative-administered quarters subsequent to the full impact of the dummy variable are almost identical, neither party was advantaged or disadvantaged with respect to the interparty unemployment performance implied by the model.

Moreover, as I stated in my article, I did not want the magnitude of my estimate of interparty unemployment performance in Britain to be influenced by the sharp rise in unemployment benefits introduced in 1966:4, which nearly all observers agreed reduced financial pressure on the unemployed to accept unattractive jobs and thereby increased average employment search time and hence the measured rate of unemployment. Similarly, I did not want the enormous fiscal stimulus associated with American intervention in the Korean and Vietnamese civil wars to influence my estimate of interparty unemployment performance in the United States, and so dummy variables for these wars were incorporated into the U.S. model. Unlike the British case just reviewed, the war dummies did serve to "disadvantage" the Democrats. Curiously, Payne does not mention this.

Payne is correct in surmising that I did not carry my time series analyses beyond 1972:4 because of the unprecedented OPEC-induced supply shock, which was simultaneously recessionary and inflationary, and the consequences of which are still being felt today. He is also correct in saying that I might have included 1973 in my estimation time range. The OPEC supply shock hit in 1973:4, but its terrible effects were not really felt until 1974 and thereafter. However, he is wrong in asserting that not including the 1973 observations critically affected my results.

Figures 1 and 2 show one-step and multistep simulation forecasts generated by the British unemployment model reported in my article (Hibbs, 1977a, Table 2, p. 1479). All of the actual unemployment observations lie within ± two standard errors of the one-step ahead forecasts and within ± one standard error of the multistep (continuous) forecasts, which is a much stronger test of the fitted model's adequacy. In other words, the model estimated through 1972:4 easily accommodates the 1973 observations. If anything, the forecasts tend to underpredict the actuals, which is quite contrary to Payne's line of argument. His conjectures about the bias introduced by excluding the 1973 data are, therefore, unfounded.

Conclusion

The monograph on which my *Review* article was based was written during 1974–75 when quantitative work in macropolitical economy was still in its infancy. It was intended to stimulate research on the political sources (especially class-linked political sources) of economic policies and outcomes as much as to establish firm propositions; certainly, I had no

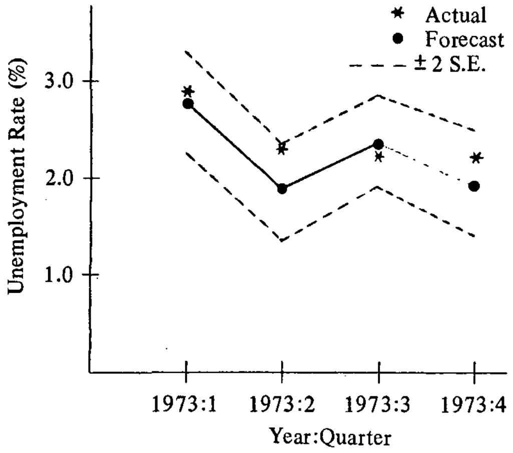

Figure 1. One-Step Simulation Forecasts from the British Unemployment Rate Model (Hibbs, 1977, Table 2)

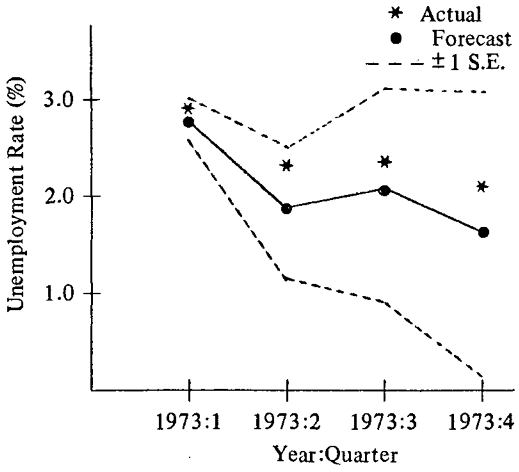

Figure 2. Multistep Simulation Forecasts from the British Unemployment Rate Model (Hibbs, 1977, Table 2)

illusions about demonstrating "iron laws." Unquestionably, the article has serious limitations. To enhance the usefulness of this exchange let me identify three that I think are particularly important.

(1) The article takes almost no account of the international economic and political constraints on macroeconomic policies and outcomes.

(2) It assumes a rather naive view of the unemployment-inflation trade-off and makes no mention of the recent "rational expectations" mini-revolution in economic theory, which has profound implications for unemployment and inflation policy.

(3) It looks at macroeconomic outcomes rather than movements in monetary and fiscal policy instruments. Only the latter, of course, are controlled directly by political officials.

In subsequent work I have tried to make some progress on these problems. I have had distinguished company. Important recent studies by James Alt, Andrew Cowart, Heino Fassbender, Bruno Frey, Robert Gordon, and Edward Tufte, among others, have advanced the field significantly.

DOUGLAS A. HIBBS, JR.

Harvard University

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The Meaning of Property Rights

TO THE EDITOR:

It is with great concern that I read Norman Furniss' "The Political Implications of the Public Choice-Property Rights School" in the June 1978 issue of the *Review.* While I agree with many of Furniss' specific criticisms, it must be pointed out that they apply more to the new welfare economics than to property rights analysis as a whole. Furniss' restricted interpretation of property rights analysis may discourage further investigations of a conceptual framework that can make potentially important contributions to the analysis of political phenomena. Two problems are apparent in Furniss' analysis.

First and most importantly, Furniss restricts the definition of property rights analysis to include only the new welfare economics. This restricted interpretation is clear in the section headings of the article where Furniss jumps from property rights-public choice to the new welfare economics as well where he uses them interchangeably in the text. This would not be too important in and of itself, but Furniss also interprets the work of others as if the two terms were interchangeable. He notes that, "for Samuels the impetus of the school was 'the effort to make welfare economics safe for the market system' " (Furniss, 1978, p. 400). Furniss' truncation of Samuels' sentence is unfortunate. The original sentence began, "the ascendency of the new welfare economics, while rooted to some unknown extent in an effort to make welfare economics safe for the market system, was grounded in more technical concerns. . ." (Samuels, 1972, p. 93). Aside from ignoring Samuels' rather substantial qualification, Furniss also seems not to realize that Samuels was referring to the new welfare economics, not property rights analysis. More to the point, Samuels' treatment of property rights is certainly not the same as his treatment of the new welfare economics (Samuels, 1972, p. 67; 1973).

The question is, to what extent does the new welfare economics overlap with property rights analysis? Unfortunately, two schools of economics employ property rights analysis. The first starts with neoclassical orthodoxy and grafts on considerations of property rights. It is to this school, better identified as new welfare economics, that Furniss' criticisms apply. The second approach starts with the critique of neoclassical analysis rooted in the institutionalist approach of John Commons (Samuels, 1973). Furniss' criticisms do not apply to this approach. All of the criticisms made by Furniss have been made before by writers in this second school. Samuels and Schmid have identified and critiqued the normative biases of neoclassical analysis and its reliance on the Coase and Pareto rules (Samuels, 1972; Samuels and Buchanan, 1975; Schmid, 1978). In these works one finds consideration of the distributive consequences of rights modifications, a broader interpretation of externalities, and a questioning of the neoclassical faith in the a priori supremacy of the market. To be fair, Furniss notes that "the refurbished classical economic theory . . . has not gone uncontested within the discipline of economics. Some have disputed the effectiveness of market solutions; others have attacked the neglect of any consideration of the structure of social power" (1978, p. 401). But Furniss fails to recognize that most of those criticisms have come from within the property rights school by those who would consider themselves property rights analysts.

Second, given that Furniss stresses the normative biases of property rights analysis, his own biases must be questioned. Furniss contrasts the property rights school to Pigouvian analysis and by rejecting property rights implicitly embraces Pigou as a guide to public policy. Furniss properly quotes Daly's criticisms of Coase's challenge to Pigou, but fails to note Daly's conclusion that Coase legitimately called into question the normative biases in Pigou. Daly concludes that:

> Writers seeking to justify or deny the relevance of collective intervention of markets characterized by external effects should concentrate their efforts not on the Coase theorem per se but rather on his view that, in practice, collective intervention may only worsen matters. As he notes, "all solutions have costs and there is no reason to suppose that government regulation is called for simply because the problem is not well handled by the market or the firm." Specifically, the likelihood of market failure does not imply government success (Daly, 1974, pp. 211–12; see also Schmid, 1978).

Furniss' implicit acceptance of Pigou and his begging the question of the dangers of big business suggest that he is not concerned with normative biases per se but with which normative biases govern political science research.

I do not know whether Furniss' criticisms arise from political science's unfamiliarity with the second strain of property rights analysis or the xenophobia that political science occasionally exhibits. More likely the problem is simply the diversity of the literature that falls under the heading of property rights analysis. The label "property rights" may not be useful as it includes elements of both neoclassical and institutional thought. Regardless of the cause, Furniss' article may discourage fuller consideration of property rights analysis. This would be unfortunate, as the second stream of the property rights literature may contribute to our understanding of politics. Most importantly, property rights analysis as practiced by institutionalist economists brings to the center of attention the inherent relationships between structure, behavior and performance, linkages that are often lost in the overly compartmentalized approaches of political science.

DAVID LOWERY

Michigan State University

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TO THE EDITOR:

I am always pleased to respond to people who take the time to comment on my work. I should advise the reader at the outset, however, that David Lowery's criticisms have only a tangential relation to the main focus of my article which is, to recall, "the *political implications of* the public choice-property rights school" (my emphasis). My reply must be similarly removed from this central concern.

Lowery has three complaints, two specific and one general. The first specific charge is that I do not quote the complete sentence from which I take Samuels' phrase "the effort to make welfare economics safe for the market system." He is correct. (Indeed, the sentence is so long—158 words with substantive footnote, 108 words without—that Lowery does not quote it in its entirety either.) Whether this action is improper, however, depends not on whether Samuels says more than I cite (which he does), but on whether what I cite misrepresents one of the things he says. The latter I do not think can be sustained. Whether Lowery does or not is hard for me to determine. In any case, I quote the final three sentences from Samuels' discussion of the "Coase Rule" (1973, pp. 145, 146) which demonstrates I believe that I have accurately portrayed *a* major theme of his paper:

> It is part of the natural history of social myths (including scientific doctrine serving as social myths) that they tend to be used wherever they will accomplish desired ends, in this case the further transformation of welfare economics to make it safe for capitalism. The Coase Rule is but one example of how the new welfare economics has chosen its assumptions and basic paradigm in such a way as to rationalize the market economy of a particular society, a return to an old tradition which Pigou dared to challenge and interrupt. Optimality in such a context is quite relative indeed.

Which brings us to the second specific accusation, that I am a secret follower of Pigou. To this indictment I plead innocent. I had thought that in my section on "the challenge of Pigou" I made clear the powerful counterarguments of Coase and Buchanan in particular. My final section in this section reads (Furniss, 1978a, p. 401), "In short, market solutions are reimplanted at the center of welfare economics." How this sentence can be read to mean that they are not reimplanted I do not know. But if others were confused I am glad to be able to clarify the issue, especially because the central focus of my article is based on the proposition that the public choice-property rights school has *succeeded* in undermining Pigou's propositions. Else the perspective would have little standing, and I have no interest in discussing bodies of thought of minor importance.

Now I turn to Lowery's general message. He begins by positing two streams of property rights literature, neoclassical and institutional, and then concludes that in addressing only the former I sow confusion (leg us pass over "xenophobic" as a rhetorical flourish) in the minds of political scientists. Perhaps because of space limitations, this position is not developed (nor is its proposition defended) very systematically. In an effort to inject some order, I would call the criticism significant were any one of the following three conditions found. (To anticipate, none are.) These conditions are as follows:

A. That I make distinctions not present in economic literature (that is, the two streams form a single river). This is the most straightforward interpretation of Lowery's letter, and it is the one most easily disposed of. To begin with, my use of "public choice-property rights" was designed to remove any ambiguities about what I was covering, which is more, secondly, than is generally thought necessary. For example, in the references following their widely cited article in the *Journal of Economic Literature,* entitled simply "Property Rights and Economic Theory: A Survey of Recent Literature" (1972), Furubotn and Pejovich list 19 separate works by either Alchian, Buchanan, Coase or Demsetz. There is one reference to a work by either Ayres, Commons, Samuels or Schmid. Similarly, they list 14 articles from the *Journal of Law and Economics* and none from the *Journal of Economic Issues.* Obviously, this is not to say that Ayres, Commons, Samuels, Schmid et al. have not done or do not do good work, merely that their contributions are seen as distinct. Finally, I had thought that this distinction was accepted (with my qualifying "public choice-property rights" label) among institutional economists as well. I refer Lowery and interested readers to Goldberg (1974).

B. That although public choice-property rights and institutional economics should be separated analytically, the position of the latter regarding the concept of property must be spelled out in any discussion because it is *the* unique, and viable, alternative (that is, there are only two streams). This notion can muster little support. I refrain from quoting numerous citations from Aristotle, Locke, Marx etc., each of whom has much that is valuable to say on the concept and rights of property, and mention only current theorists. Here we find property rights an essential element in the thought of figures as diverse in their normative conclusions as Rothbard, Nozick, Buchanan, Rawls, and Macpherson. (I pick these because they happen to be discussed in a very useful paper by Lehning, 1978.) We also find well-developed political positions based on a rigorous delineation of appropriate and inappropriate property rights. The most impressive effort, I feel, has been made by a group I term "functional socialists," whose arguments can be traced to R. H. Tawney and Ernst Wigforss (see Furniss, 1978b). Let us add that institutional economists contribute many insights into the nature of property rights as well; Lowery provides a list of references. The point, of course, is that all these contributions are best treated in *separate papers or articles* (as indeed I have done) or in a monograph detailing the implications of divergent perspectives on the concept of property. In this connection, a piece by Lowery that would argue that institutionalist economists "may contribute to our understanding of politics" might well be in order. But it is neither necessary nor relevant for me in the midst of a discussion of the political significance of the public choice-property rights school to pluck from among the myriad formulations of the concept of property those of institutional economics for sustained treatment. Unless it is claimed. . . .

C. That although distinct from the public choice-property rights school and only one of many perspectives on property, institutional economics has so eclipsed "neoclassical" economics that (to mix metaphors) institutional economics has become a mighty river while neoclassical economics has been reduced to a small creek. In this case one might declare (as Lowery at times seems to do) not that I should have incorporated both perspectives but that I have written the wrong article. I will not belabor the obstacles facing anyone attempting to argue seriously for this proposition, as opposed to merely stating it. Fortunately, we need not investigate what might be meant by the statement that "property rights analysis as practiced by institutionalist economists brings to the center of attention the inherent relationships between structure, behavior, and performance." There is no need for invidious comparison; for our purposes it is enough to note that the importance and intellectual influence of the public choice-property rights perspective can be exaggerated only with difficulty—the pages of this *Review,* for example, are full of debts, both implicit and explicit, to its basic conceptual thrust. Briefly, I refer the reader to the able presentation of Ostrom (1977). See also his citations. I would be glad to provide further references.

In sum, I continue to feel that my delineation of the public choice-property rights perspective is appropriate and that my argument is *potentially* significant. (Since Lowery states that my "criticisms apply" to the school, his letter does not offer much opportunity to say more than this.) I conclude on a conciliatory note. Scholars working in both the traditions of public choice-property rights and institutional economics share a disdain for mere "data manipulation" and a concern for elaborating the essential issues of economic and political life. James Buchanan puts the matter very well, and it is proper that he should have the final word (1976, pp. 167, 168):

> The hard questions are not readily formulated in terms of testable hypotheses. But this offers no cause for not thinking about such questions, for not discussing them, for not searching for an appreciation and understanding. . . . We do not "solve" the "problem" of social order by producing a unique "solution," regardless of the sophistication of empirical techniques. There is no objective "truth" to be established here. The "problem" of social order is faced eternally by persons who realize that they must live together and that to do so they

must impose *upon themselves* social rules, social institutions. Economics and economists cannot evade their responsibility in the continuing discourse over such rules and institutions by shifting attention to trivialities. To the extent that they do so, their functional roles can only be filled by the charlatans and the fools, whose presence around us requires no demonstration.

NORMAN FURNISS

Indiana University

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TO THE EDITOR:

As a person who once wrote a dissertation on Locke, I was intrigued to read Robert H. Horwitz's review of the Clarendon edition of Locke's *Essay Concerning Human Understanding (APSR* 72:651–52). I was especially struck by the account of the difficulties Locke had with printers. Given Horwitz's statement that "the Clarendon edition has been suspiciously inaugurated" it seems that the spirit of the seventeenth century lives on in printing as well as in philosophy.

JOHN O'CONNOR

American Philosophical Association

EDITORIAL NOTE

The following articles have tentatively been scheduled to appear in the June, 1979, issue:

David O. Sears, Carl P. Hensler and Leslie K. Speer, University of California, Los Angeles, "Whites' Opposition to 'Busing': Self-Interest or Symbolic Politics?"

Michael Johnston, University of Pittsburgh, "Patrons and Clients, Jobs and Machines: A Case Study of the Uses of Patronage"

Richard L. Cole, George Washington University, and David A. Caputo, Purdue University, "Presidential Control of the Senior Civil Service: Assessing the Strategies of the Nixon Years"

Edward T. Jennings, Jr., State University of New York, Buffalo, "Competition, Constituencies, and Welfare Policies in American States"

David John Gow, Rice University, "Scale Fitting in the Psychometric Model of Judicial Decision Making"

Arend Lijphart, University of California, San Diego, "Religious vs. Linguistic Class Voting: The 'Crucial Experiment' of Comparing Belgium, Canada, South Africa, and Switzerland"

David M. Lampton, Ohio State University, "The Roots of Interprovincial Inequality in Education and Health Services in China since 1949"

William Pang-yu Ting, University of Michigan, "Coalitional Behavior among the Chinese Military Elite: A Nonrecursive, Simultaneous-Equations, and Multiplicative Causal Model"

James M. McCormick and Young W. Kihl, Iowa State University, "Foreign Policy–IGO Linkages: Some Empirical Findings"

Timothy A. Tilton, Indiana University, "A Swedish Road to Socialism: Ernst Wigforss and the Ideological Foundations of Swedish Social Democracy"

Alan Gilbert, University of Denver, "Social Theory and Revolutionary Activity in Marx"

An unfortunate combination of errant typesetting and proofreading resulted in misplacing sections of Professor J. Patrick Dobel's article, "The Corruption of a State" (September, 1978, pp. 958–73). We are most apologetic, understanding how upsetting this type of error is to authors. We reprint below the whole section of Professor Dobel's article in which the offending misplacement appeared (ref. pp. 970–71).

Education: Formal, Family, Religion and Militia

Inequality dominates the causes of systematic corruption, but human nature must also be addressed. Education and socialization must inculcate disciplined commitment to other citizens and loyalty to the commonweal.[49] Customs, habits and mores can sometimes be strong enough to sustain institutional integrity and loyalty among citizens even after great inequality exists. Education and socialization, however, fight a rear-guard action. Neither equality without education nor education without equality can sustain a just, stable and equal state. Corruption spreads beyond the political realm and cripples the structures which generate reasonably disinterested loyalty and civic virtue. As relations become instrumentalized under the pressure of inequality, citizens lose the capacity for piety, dutifulness and affectionate loyalty. Four vital areas of political socialization are undermined: formal education, the family, organized religion and mutual self-defense.

The society's civic educational system is corrupted by several onslaughts. As the corruption of values in government and the wider society becomes more apparent, it becomes harder to find teachers who can seriously teach these values. Teaching, itself, becomes an undervalued occupation in a world of great economic and social disparities, and fewer talented people enter it. Additionally the teachers and schools come under constant attack from various factions for teaching a set of values which might lead a student to question a particular faction's place in society or damage a faction's future recruitment. The schools also confront students and parents who see that the "older" concern with rational and humane mores and loyalty are counterproductive in a world of atomized selfishness and factional competition. The schools are slowly transformed into nothing more than occupational training for the factions and become devoid of any independent values linked to loyalty to the common good and other citizens.

The incapacity for loyalty also wrecks the social stability of the family. The loyalty of husband and wife lasts only as long as it is convenient; adultery and divorce become normal and justifiable whenever duties of fidelity interfere with immediate pleasures. As the parents liberate themselves, the children are neglected or shunted off because they seem unrewarding.

The lack of loyalty and care in the family destroys the family as a socializing agent. In families citizens acquire basic moral beliefs and learn rudimentary forms of justice, cooperation and affirmation of authority.[50] As parents betray one another and lose confidence in their authority, children learn to ignore parental authority and pursue their own interests. Individuals learn to perceive all law and morality as oppression.[51] If children have no respect for rules given by parents, they will never accept laws which impinge upon them for the benefit of others.

The corruption of organized religion destroys another voluntary organization which sustains moral commitments to others.[52] The change is not so much one of religiosity as of piety. The moral claims of religion to limit avarice or encourage charity lose their force. Fear of God wanes and the self-sacrifice of piety is outweighed by love of gain.

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[49] Plato (1957, 386a–416c; 423e–424c); Aristotle (1962, Bk. 2, Chs. 7, 8; Bks. 7, 8); Rousseau (1964, *Du contrat social,* Bk. 2, Chs. 6, 12; *Discours sur l'economie,* pp. 260–61).

[50] Machiavelli (1965, *Discourses,* Bk. 1, Chs. 11–15; Bk. 2, Ch. 2; Bk. 3, Ch. 33); Rousseau (1964, *Discours sur l'economie,* pp. 261–62).

[51] Plato (1957, 553a–553e; 562e–565e).

[52] Machiavelli (1965, *Discourses,* Bk. 1, Chs. 11–15; Bk. 2, Ch. 2; Bk. 3, Ch. 33); Rousseau (1964, *Du contrat social,* Bk. 2, Ch. 7; Bk. 4, Ch. 8).

The decay of religion occurs on two levels. First, citizens slowly leave the churches or transform them into purely social or private activities. Second, the church itself becomes a faction. To maintain its institutional power it might ally itself with the elite and then act as an agent of control rather than one of grace and worship. The constant vacillation of the Delphic oracle among the various Greek factions reflects such bankruptcy. The religion might also follow the strategy of the Roman Catholic Church of Machiavelli's or Rousseau's time and use its spiritual authority to gain riches, land and power for itself while sacrificing the moral integrity of its leaders and the spiritual welfare of its members.[53]

Religion's inherently mysterious and evocative relation with people gives it the constant potential to renew the moral life of the community. Its clergy can be corrupted, its membership thinned, but the possibility of prophecy and regeneration remain. The resurrection of Florence under its unarmed prophet, Savonarola, and Geneva's transformation by its armed prophet, Calvin, were classic examples of religion's "restorative" powers.[54]

The increasing dissolution of the citizens' bonds of loyalty ends the state's ability to generate its own militia. In a just and stable state a voluntary citizen army served three purposes. First, it was a counterweight to the rich and powerful. As long as the citizens controlled the main source of legitimate coercion and defense, the loyalty of the elites was reinforced by fear of arms. Second, loyal and committed citizens made better and less ambitious soldiers. Third, a participatory militia was a great equalizer. It pulled all classes of society together and made it more democratic in its values and reinforced the loyalty of citizens for one another.[55]

In an unequal and corrupted state the bulk of the citizenry have little reason to defend a state which gives them so little. The elites care too much for themselves and possess their own means of protection. They also fear to see the poorer citizens armed. The state is reduced to expedients for defense: payoffs to enemies, mercenary soldiers, wars by proxy and a professional army. The bribery scheme works in the short run, but it is too dangerous in the long run and often generates internal unrest because of the humiliation and cost involved.[56] Mercenaries, like Francisco Sforza, The Duke of Milan, are inefficient, expensive, often disloyal and liable to turn on the country and conquer it.[57] Proxy wars, as the Athenians discovered in trying to rule their empire indirectly, are extremely costly and they usually involve unreliable allies and pull the state into increasingly larger and costlier intervention.[58] The last solution, the professional army, is much more militarily efficacious, but it poses a great threat to internal freedom. The army is loyal to those who pay it and can easily become an adjunct to the ruling classes. The maintenance of a standing army involves larger budgets and creates many opportunities for corrupt alliances between the military and various economic factions which supply it. Finally, if the army should develop its own inner cohesion, the army can become the most powerful faction in the state. The state can either buy it off with great sums of money or the military faction may sell itself to a political entrepreneur or simply take over the government.[59]

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[53] Thucydides (1934, Bk. 1, Chs. 5, 6); Machiavelli (1965, *Discourses*, Bk. 1, Ch. 12; *History of Florence*, Bk. 8, Ch. 17; *Prince*, Chs. 7, 11, 12); Rousseau (1964, *Du contrat social*, Bk. 4, Ch. 8).

[54] Rousseau (1964, *Du contrat social*, Bk. 2, Ch. 7); Machiavelli (1965, *Prince*, Ch. 6; *Discourses*, Bk. 2, Ch. 16; Bk. 3, Chs. 1, 24; Letter 3, Vol. 2, pp. 886–89).

[55] Machiavelli (1965, *Discourses*, Bk. 2, Ch. 10; *Art of War*, Preface; Bk. 1); Rousseau (1964, *Considérations sur le Pologne*, pp. 1012–20).

[56] Machiavelli (1965, *Discourses*, Bk. 2, Chs. 10, 30).

[57] Machiavelli (1965, *Prince*, Chs. 12, 13; *History of Florence*, Bks. 1–6, passim, esp. Bk. 1, Ch. 39; Bk. 4, Ch. 24; Bk. 5, Ch. 34; Bk. 6, Chs. 1, 20).

[58] Thucydides (1934, Bks. 3–8, passim, esp. Bk. 3, Chs. 10, 11; Bk. 5, Ch. 16; Bk. 7, Ch. 21; Bk. 8, Chs. 24–25; Bk. 1, Ch. 4).

[59] Machiavelli (1965, *Art of War*, pp. 566–76; *Prince*, Chs. 6, 12; *Discourses*, Bk. 2, Ch. 12); Rousseau (1964, *Discours sur l'economie*, p. 269).

BOOK REVIEWS

Political Theory and Methodology

Walter Lippmann. By Larry L. Adams. (Boston: Twayne Publishers, 1977. Pp. 229. $8.50.)

Writing in *The Nation* in 1933 Amos Pinchot declared: "Not the least of the benefits of reading Mr. Lippmann is that he can be quoted on either side of almost any question." Since then many others have accused him not only of inconsistency but of abandoning the liberal convictions that characterized his youthful writings. More sympathetic readers describe the changes in his thought as evidence of developing intellectual maturity. While acknowledging inconsistencies in his thought Larry Adams discovers an intellectual and moral coherence in all of Lippmann's writings.

Rather than betraying the liberal tradition, Lippmann, in Adams' view, sought to preserve and perpetuate the best in the liberal tradition while revising his thinking in the light of experience. "The Liberal tradition is a house of many mansions, and it sometimes seemed Lippmann was bent on dwelling in all of them—as socialist, Progressive, diplomatist, skeptic, moralist, advocate of *Realpolitik*, natural lawyer. Lippmann's ultimate beliefs were partially concealed by this succession of analytic postures. But none of his stances is inconsistent with his commitment to Liberalism as a political method—a system of limited government and individual liberties, of elections and representation, persuasion and compromise" (p. 184).

In Adams' view Lippmann never succumbed to ideological thinking nor was he an active participant in ideological movements. He was a genuine political theorist who did not shrink from exploring and evaluating the epistemological and moral foundations of the liberal tradition to which he adhered. But apparently, from Adams' perspective, this exploration never shook Lippmann's confidence in the ultimate political wisdom of the liberal tradition. Three of Lippmann's teachers at Harvard (William James, George Santayana and Graham Wallas) had a profound influence upon his thinking throughout his life. Lippmann struggled intellectually throughout his life to combine the insights of Plato, Freud and pragmatism. "Lippmann struggled to find a hopeful prescription for Liberal society. He took up the old Liberal theme, reworked by Freud, that the development of society is a process analogous to personal growth and that the surmounting of institutional religion creates a situation analogous to the crisis of adolescence. Lippmann echoed Freud's hope that a new mature human type would emerge after the trauma of unresisted disbelief had passed" (p. 196). But Lippmann's former teacher, Santayana, was dubious. Writing a review of *A Preface to Morals* in 1929 Santayana said: "It would be interesting to hear" what Lippmann "forsees will be the ruling passions, favorite pastimes, and dominant beliefs of mankind when the hitherto adventurous selfish human animal has become thoroughly socialized, mechanized, hygienic, and irreligious" (p. 125). Santayana went on to say: "The virility and chivalry of virtue lies precisely in being inflexibly true to oneself, although all other people may be different and one might have been different too. I commend this reflexion to those who feel safe in their ethics and politics if they think they are swimming with the tide—a form of cowardice peculiarly modern and peculiarly short-sighted" (pp. 125–26).

In his early work *A Preface to Morals* and in his most recent *Essays in the Public Philosophy* Lippmann grappled with problems that lie at the heart of our present discontent. If, as Larry Adams says, "Walter Lippmann's writings do not constitute a comprehensive system of thought," nevertheless "part of the pleasure of reading any political theorist is gained from accepting the author's tacit invitation to engage in a dialogue" (p. 184). Lippmann was and is a stimulating intellectual protagonist and Adams' book is a good introduction, for those who need it, to Lippmann's liberal odyssey. Adams is quite right, I think, in suggesting that in reading Lippmann's books we are following at least some of the major aspects of his intellectual autobiography. "Lippmann's program for society transformed was a representation of his own inner life. To make conscious our purposes, to refine our instincts, to socialize our motives, to exert mastery over the future through reasoned choice and plan—these aims were the heart of his conception of social reform. The theorist's values always guide his perceptions and description of reality, he wrote at another point. An admirer of Plato, Freud, and James, Lippmann would necessarily be sensitive to this understanding" (pp. 16–17).

JOHN H. HALLOWELL

Duke University

Justice and Punishment. Edited by J. B. Cederblom and William L. Blizek. (Cambridge, Mass.: Ballinger Publishing Company, J.B. Lippincott, 1977. Pp. 222. $16.50.)

It is hard to see for whom this collection of essays, produced for a symposium at the University of Nebraska, is intended. It won't do for students or the general public: the articles differ too much in quality, subject and level of discourse. It won't do for scholars or professionals either, since the essays do not break new ground.

The longest essays are also the worst. J. B. Cederblom's introduction is serviceable, though his style is awkward. John Hospers lengthily and carelessly goes over well-ploughed ground, and manages to get too many things wrong. Thus, after rejecting vengeance as motive for retribution, he writes, "motives other than the desire for justice are irrelevant [to the justification of punishment]." That motive is irrelevant too: punishment may be just and/or useful regardless of what motivates it—be it even a desire for injustice. Motives (why) as distinguished from acts (what) and intentions (what for) cannot be relevant to justification. Later Hospers states, "the desert theory in general does not assert" that Eichmann "deserves" a punishment worse than death. Hospers gives no reason for this statement, but adds "even the mirror image theory cannot really assert [that Eichmann deserved worse that death] for one man cannot die five million deaths." That does not mean that he cannot deserve them; Hospers confuses what may be deserved with what is possible, practical, or decent to do. The scope of punishment—but not of desert—is more limited than the range of crime, because some punishments are not practical, or however deserved, are repulsive to us (e.g., torture). Hospers rightly thinks that restitution cannot take the place of punishment. But he omits the major objections: (1) it would license the rich to commit crimes (unless punishment is reintroduced); (2) it would foster crime generally (a burglar threatened with no more than restitution would find the risk worth taking). Hence (3) whatever its merits, restitution is irrelevant to the issue it is supposed to address. Restitution cannot protect society, nor be just, since rather than punishing, or incapacitating, or deterring offenders, it simply compensates victims. Hospers goes on to discuss earnestly a theory attributed to one Lefevre which advocates self-protection and urges that offenders should not be bothered if successful. Hospers' refutation of this "theory" is on the level of the theory itself—which takes some doing (and the absence of a sense of humor).

Unlike Hospers', Hugo Adam Bedau's essay does not offend. But it takes too long to state the unquestioned: a theory of punishment must require punishment for guilt. It hasn't been news since Kant. Neither is Bedau's argument. David Fogel has the merit of having stated his well-known theories of corrections when his view was less fashionable than it has become, but this merit does not justify stating it again and again—while ignoring objections (which I have no space here to list). This goes as well for Norval Morris' well-written essay on corrections. Morris opposes coercive rehabilitation but does not deal seriously with the philosophical issues involved (isn't a correctional institution to correct?). He favors voluntary rehabilitation on pragmatic grounds, but does not tell why it is a social duty to offer appropriate opportunities, nor what reason he has to believe that it will yield a sufficient return. (I can't see any). James Wilson summarizes the practical political issues surrounding punishment with his usual concise and dispassionate lucidity.

There remain three philosophical essays. Martin P. Golding competently worries the usual dilemmas, but he does not carefully analyze the questions he attempts to answer. Thus he fails to discover that "special deterrence" does not differ from rehabilitation. Edmund L. Pincoffs, focusing more narrowly on whether questions of desert are decidable, gives a sophisticated analysis based on the appropriate definitional distinctions. His affirmative answer is refreshingly direct and quite persuasive. Richard Wasserstrom's essay, finally, touches on all the right questions but he makes a muddle of the answers. Even though he seems to see the need for the distinction, he does not really separate punishment as an institution, necessarily deterrent in purpose, and the distribution of punishments to individuals, justifiable only on retributive grounds. I don't think that such confusion is excusable.

ERNEST VAN DEN HAAG

State University of New York, Albany

The Philosophical Anarchism of William Godwin. By John P. Clark. (Princeton, N.J.: Princeton University Press, 1977. Pp. 320. $16.50.)

William Godwin remains today a kind of sad and solitary figure, a theorist of considerable importance whose work has generally been neglected or unappreciated by students and scholars of modern political theory. However, there appears today to be the slightest hint of a reawakening of interest in Godwin, and John P. Clark's welcome volume is a substantial contribution to the reappraisal of Godwin's position and significance.

The 1793 *Enquiry Concerning Political Justice* has long been regarded as the "first theoretical exposition of anarchism," and writers who bothered to concern themselves with anarchism in the first place have always deferred to Godwin's work as its first systematic presentation. Yet Godwin's importance is much broader than his role in contemporary anarchist thought. In Godwin we find perhaps the major crossroads of liberal thought where themes of liberal individualism, rationalism and utilitarianism intersect with nascent libertarian and socialist thought.

Clark's claims on Godwin's behalf are more modest than mine. He makes a convincing case for according the English philosopher a "much more prominent place in the history of utilitarianism," and regards him as a solid rival to John Stuart Mill in defense of freedom of thought and expression. Godwin not only preceded Mill by a half-century but also offered a defense of more comprehensive depth and dimension. Of course, Godwin's contribution to libertarian theory is correctly characterized as "enormous."

Clark's work is well conceived, beginning with a consideration of Godwin's first principles: his belief in the supremacy of reason, his doctrine of necessity and his particular conception of human nature. Godwin's thought is far from simple and Clark labors to illuminate many of its apparent paradoxes with considerable skill. For instance, Clark must explain how a great libertarian such as Godwin can hold that a determinist philosophy is more conducive to moral improvement and a stronger sense of personal responsibility than is a doctrine of free will.

Godwin is a utilitarian, with a social interest: to him the matter of "general benefit" is of greatest importance. Godwin's is an ethics of duty. At first glance this might appear odd for an anarchist—but only for those who have accepted the caricatures of anarchism and not dealt with it as a moral philosophy. One's moral duty, Godwin believes, is determined by the principle of utility: to produce the greatest good for the greatest number. Godwin holds that "it is impossible for me to perceive that an end is good, and yet fail to desire it." The better informed we are as to cause and consequence, the more we understand the laws of nature and society, the more accurately we can perceive those ends which truly are good. Ultimately this is a matter of private judgment; the individual's strength of character, sincerity, consciousness and ability to reason become the guarantee of social order. Human justice can arise only from moral duty freely conceived and fully comprehended. One cannot relinquish any share of one's duty and responsibility to any representative or agency. Thus Godwin's preoccupation with individual autonomy: in short, his anarchism.

Clark's work is based not only on familiarity with the several editions of *Political Justice*, often considerably revised, but also with the full corpus of Godwin's work. One has to go elsewhere for an understanding of the grounding of Godwin's project in the dissenting tradition, in Enlightenment individualist and rationalist thought. Nor does Clark's book attempt biography: this is unfortunate, because for a thinker as neglected as Godwin, an additional 20 or 30 pages would have distinctly enhanced the book's value. On the other hand, Clark has done a commendable job of relating Godwin's thought to concerns of our day, while simultaneously correcting numerous misconceptions both as to Godwin's ideas and anarchist thought in general. In particular, Clark's emphasis on the distinction between state and government does much to counter the stereotyped caricature of anarchism as a negative, naive and utopian philosophy.

Godwin was a practical political philosopher who found government eminently useful in certain instances: it, too, must be evaluated according to the principle of utility. Anarchism's goal, after all, is self-government, true self-management in human society. Godwin elaborated one of the earliest and fullest rationales for "a decentralized, federated system of social organization." The coercion and disordering of violent revolution he saw as more likely than not to inhibit the development of critical faculties of reasoning; the true revolution was one of consciousness. Any form of political action which attempts to lead people, to engineer them into forms not of their own making, is injurious. New political structures will prove fruitless unless they emerge socially from changes in individually held moral attitudes.

Goodwin offered no simple solutions. Though opposed to philosophical egoism, Godwin was a fierce individualist and probably failed adequately to emphasize the necessary social context of individual action. It was thus left, as Clark points out, to later libertarian theorists to take up Godwin's leads. Godwin's analysis nevertheless not only foreshadows but often goes directly to the heart of many of the most challenging issues of our time: "worker self-management; neighborhood government; decentralized planning, administration and decision-making; open education and deschooling" to say nothing of the broader enduring themes of political theory.

WILLIAM G. NOWLIN, JR.

University of Lowell

Hobbes and America: Exploring the Constitutional Foundations. By Frank M. Coleman. (Toronto and Buffalo: University of Toronto Press, 1977. Pp. ix + 159. $12.50.)

Hobbes: Morals and Politics. By D. D. Raphael. (London: George Allen & Unwin, 1977. Pp. 104. $14.50, cloth; $6.50, paper.)

Coleman might better have called his book Hobbes *in* America; he seems to be all over the place. First, there is Madison, more Hobbesian (i.e., ruthless) than the Sage of Malmesbury himself; then Hamilton, who takes a brief lateral when Madison trips up at scrimmage; then Calhoun, a middle line-backer, sacking the quarterback; Thoreau, always the ultimate individualist, fumbles; and William Graham Sumner coaches the replacements to all charge the line at once, only to have them penalized for "encroachment" by the omnipresent sovereign referee. And finally, Robert Dahl, by a blackboard in the locker room, teaches future referees the gentle, mindless art of conflict management. Everyone thinks the game was invented by Locke, but if they look closely enough, they will see that Hobbes thought of it first.

Hobbes and America is not about football, however; it is a polemical exercise in demonology. We are on the brink of the state of nature but don't know it: the *litterateurs* of American politics—the "erroneous reformers" and the "apathetically silent 'pluralist-empiricists' "—fail to divulge the character of our political experience because they don't understand how it is shaped by Hobbes. And scholars have been misled about the intellectual underpinnings of American politics "because of Hobbes's notoriety and the complexity of his real argument" (p. 68). Coleman's purpose is to "tell the truth about the American polity" (p. 38). The truth is, the "operative realities" of American politics come not from *The Federalist,* not from Locke, but from Hobbesian constitutional philosophy. These "realities" are, transactional relations, conflict management, ruthless individualism, and a "merely policing sovereign" (*passim ad nauseam*). Hobbes is a liberal democrat, and he is the "parent source" of American political thought, from Madison right down to Truman, Neustadt, and Dahl.

Hobbes' "real" argument will be appealing to those who have suspected Hobbes of being a closet liberal. I list some of the highlights: Hobbes is not an absolutist, and he is "an enemy of authoritarian rule" (p. 91). He is a revolutionary political thinker whose views were "of critical importance in shaping the events of 1642–46, 1688, and 1776" (p. 55). The right of resistance and the concept of inalienable rights get their best expression in Hobbes, rather than Locke—*especially* the inalienable right to property (pp. 78–83). The sovereign is to manage, not suppress, "social" conflict. He will avoid the use of the sword, and will even avoid threats of coercion—when he is in conflict with individuals and groups, bargaining and negotiation will be the essence of the process. The source of his power is the covenant, but it imposes limitations on him. Public order is sustained (Coleman "infers") through transactional relations which adjust the "absolute" right of the sovereign to the right of the individual; since force or the threat thereof "is unsatisfactory," the sovereign and subject "deal in the mutual coin of self-interest"; indeed, the subject can bribe the sovereign, if it is not strictly forbidden by law (Hobbes does say as much), and the sovereign can exploit the powers of his office for private needs or for "the needs of clientele groups vital to his success in office" (pp. 86–87). According to Coleman, other critics of Hobbes "grossly exaggerate" the sovereign's power. Actually, the sovereign's power is always being created, but is never realized fully; he will negotiate from a position of weakness, at least with parties as wealthy as he, and he will be "completely helpless" in dealing with the "rampant paranoia of a liberal society." In such circumstances, Coleman almost says, fear will be crucial, only it will be the *sovereign* who is afraid of his subjects.

There is more, of course: neither Locke nor Madison can be understood properly without interpolation from Hobbes. Despite separation of powers ideas and dual sovereignty (Madison), both see the state as absolute and unitary (pp. 115, 126). Locke transmitted an "ungraceful" and "lackluster" version of *Leviathan* to America, and Madison implemented it. While lackluster, Locke's *Leviathan* is more extensive because it is social, and not just political: it "superintends the innermost details of our lives from childhood" (p. 114). Calhoun, Thoreau, and Sumner are surrogate fathers, transmitting the liberal-democratic heritage of secessionist politics, absolute subjectivism, and a strong state.

Those interested in seeing how these conclusions are reached should consult the book, though they will be disappointed. Coleman attempts far too much in such a short book, and his critical apparatus is flawed. Quotations are few, paraphrasing often distorts, almost everything is out of context, sources are uncritical, and some are used as straw men (e.g., p. 88, n. 9). Evidently uncertain of his argument, Coleman engages in occasional *ad hominem* attacks which I find distasteful. All of this is unfortunate, because Coleman's Hobbes is certainly worth a clear, tight exposition. As it is, the demonology leads nowhere.

Raphael's work is intended as a "genuine introduction" for the elementary student, though it is not (as he says) an elementary statement. As such, it touches briefly and lucidly on Hobbes' method, metaphysics, psychology, and morals. The discussion of political theory, however, is confined to Raphael's own interesting contribution to the obligation controversy ("a combination of two kinds of obligation, natural and artificial"), and slightly over a page elsewhere, half of which uses Locke to illustrate Hobbes on consent. The latter is unsatisfactory, particularly in an introductory book, but it is characteristic of philosophical treatments of Hobbes, in which conceptual analysis tends to excite the intellect at the expense of one's political and literary imagination. The lengthy survey of interpretive works at the end is finely critical and sympathetic; Raphael thinks Hood's *Divine Politics* should be taken more seriously than it has been. I concur.

Both of these works are especially valuable for early graduate teaching, Raphael's because of the survey of other works on Hobbes, and Coleman's because it shows how poor scholarly and literary techniques can muck up a potentially interesting argument. Polemics are fine, as long as they are done with a sense of method and style; *Leviathan* teaches at least that.

RICHARD ALLEN CHAPMAN

University of Montana, Missoula

Equality and Freedom: International and Comparative Jurisprudence. Vol. I, II, and III. Edited by Gray Dorsey. (Dobbs Ferry, N.Y.: Oceana Publications, and Leiden, The Netherlands: A. W. Sijthoff, 1977. 3 Vols. Pp. xxx + 1203. $75.00.)

These volumes comprise the 105 papers presented to the 1975 World Congress of the International Association for Philosophy of Law and Social Philosophy, held in St. Louis. Seventy-five of the papers are in English; for the rest (in French, German, or Spanish) English abstracts are supplied.

The program was organized around problems rather than concepts, so that contributors would approach philosophical issues in terms of matters about which they had concrete knowledge. To be sure, some papers were wholly conceptual; and others were lacking in philosophical content; but most combined the two. Some of the best papers are primarily conceptual, while a few that fall in this category are too abstract to convey much meaning, at least to the American scholar.

Five sections, comprising just under half of the total pages, definitely deal with problems. They are "participation," "anticipation" (with two subsections: one devoted to "environment and natural resources" and the other to "information science and human relationships"), "the scientific manipulation of behavior and legal protection of freedom," "new legal institutions for new social relations," and "equality among nations."

Among the three sections that invite a more conceptual approach, the first, entitled "Persons," is the longest section in the whole work. The other two deal, respectively, with "tensions between the goals of equality and freedom," and "equality and freedom: past, present, and future." The line between these two sections is not sharply drawn. In the second, for instance, one finds an excellent analysis by Alistair Macleod of the ambiguities in the concept of equality of opportunity.

Not surprisingly, the single theme most evident throughout the volume is that of individualism versus a socialist approach. Sometimes the line thus drawn is between scholars from Eastern Europe and other socialist countries (e.g., India, Israel), on the one hand, and the rest of the world, on the other; but by no means always so. In fact, it would appear that the balance of the whole work is tipped more in the direction of a socialist or collectivist approach than otherwise.

The initial article, by Luis Recaséns-Siches of Mexico, is written from a strongly individualistic point of view. It is much more all-encompassing than the title of the section ("Persons") would suggest; it is, in fact, an introduction to the whole work. Recaséns-Siches insists that "the collectivity has no substantial reality" (p. 7) and that, while people need the help of government and law, "only the individual human being, who enjoys freedom, can develop his own creative forces" (p. 10). He goes on in detail to elaborate the implications, especially the legal ones, of this starting point. In stark opposition to this view, the Bulgarian scholar Peter Popoff opens his discussion of "The Full Development of Personality in Socialist Societies" with the statement that "the concept of 'person' is not primary . . . in understanding the relations of society and state." Rather, "such a primary concept is an illusion or an intentional myth of the bourgeois ideologists of the contemporary capitalist world" (p. 245). A more extended and less tendentious representation of a similar point of view is set forth by Karl A. Mollnau, of the East German Republic, who contends that personal freedom and discipline under socialism are not opposed (p. 156). One of the most subtle and morally sensitive discussions in this section is that of the late John Plamenatz. An individualist who yet insists upon the social nature of the person, he notes that problems arise both from the claims of persons "merely as persons, regardless of social status, role, or occupation" and "those [problems] that turn on the need that a person has (or is alleged to have) to achieve a satisfying sense of identity, an idea of himself as he is and aspires to be that is acceptable to him, and that gives him self-confidence and self-respect and the sense that life is worth living" (p. 48). A more strongly individualistic and legal point of view is expressed by Iredell Jenkins.

In the two sections dealing directly with equality and freedom, while the majority of the contributors find tension between these two concepts, some argue to the contrary. This position is taken by Shlomo Avineri of Israel, Adam Lopatka of Poland, and Monique and Roland Weyl of France. The initial article, by D. D. Raphael of England, takes a moderate position on this issue, arguing that tension does exist, but that it results largely from natural, not artificial, inequality, while it is the latter kind of inequality that is in fact much more extensive in society. In the first of these sections, the libertarian position finds its sole proponent in these volumes in Tibor Machan. Governments should see that persons are equal in citizenship, he contends, but to extend their intervention beyond this point, such as by establishing equal pay for equal work, is beyond their proper province (p. 667). In the second of the sections on liberty and equality Macleod analyzes the ambiguities in the concept of equality of opportunity. The articles by Lopatka and the Weyls make similar points.

Turning to the problem-oriented papers, first we have the 13 papers dealing with participation. Breezily put, they generally think it is a Good Thing, usually being careful to note the many varieties of political participation, often finding the least formal kinds most significant. Michael Bayles' article stands out for its contrary point of view that "personal participation is no longer a generally viable ideal" (p. 357). His argument is supported by Ekkehard Klausa of Berlin, who discusses lay participation in courts of law, on the basis cf an empirical study of this system.

Stanley Benn of Australia leads off the discussion of "environment and natural resources." He argues, contra some environmentalists, that the "humanist-personalist" ethic need not be blind to the values of nonhuman objects, while still holding that the burden is on anyone who would put the biosphere before persons. Articles in the subsection devoted to "information science and human relationships" leave the impression that the problems in this sphere are less serious than those in the preceding subsection (which included, for instance, that of overpopulation).

The section on scientific manipulation of behavior (including physical, psychological, and propagandistic manipulation) is composed of nine articles. Samuel Shuman, dealing with physical (largely medical) manipulation argues that the major controls in defense of freedom should be carried out by the organized professions themselves. Others insist upon the importance of informed consent. Gyan Sharma concentrates on "Freedom as an Operational Concept." What is most to be avoided, he contends, is the zeal of the perfectionist, some of which he finds, à la Skinner, in the literature of freedom and dignity. He calls for a contextual approach.

In the section on "new legal institutions for new social relations," Mihailo Marković provides an interesting and fairly concrete discussion of decentralization and industrial democracy in Yugoslavia; and Geoffrey Marshall sets forth a painstaking analysis of the ambiguities of "equality under the law," as illustrated by two British statutes and their enforcement.

Finally, the eight articles on "equality among nations" are introduced by Oscar Schachter with a good overview of economic inequalities among nations. Ali Mazrui, of Kenya, advances the interesting argument that technological changes are moving international interdependence from the "feudal-imperialistic" stage to the mature stage, where interdependence and equality march side by side.

Although these 105 articles are not equally relevant to the announced topic, their extensive application of general theory to concrete problems gives them special distinction.

Gray Dorsey did a skillful job of organizing and editing these articles, many of them written by persons for whom English is a second language. He is also to be thanked for providing a helpful index.

J. ROLAND PENNOCK

Swarthmore College

Taking Rights Seriously. By Ronald Dworkin. (Cambridge, Mass.: Harvard University Press, 1977. Pp. 293. $12.00.)

Taking Rights Seriously is important because it pulls together into a single volume the thought of one of the most creative minds in the field of philosophical jurisprudence. While the book is significant as a compendium of Ronald Dworkin's writings, it suffers somewhat from the inevitable lack of continuity in a collection of essays written over more than a decade.

The book divides into two parts. Dworkin first sets out to describe his own normative theory of jurisprudence which he proposes as a preferable alternative to positivism. The target of his attack is the particular version of legal positivism expounded by Hart in *The Concept of Law* and other essays. Dworkin's charge against Hart's positivism is that the theory does not "take rights seriously." Rights, Dworkin argues, in the framework of legal positivism, are determined by judges in the exercise of judicial discretion. Within the broad scope of discretion allowed by legal positivists, there is a failure to give recognition to general standards or principles, e.g., "No man may profit from his own wrong." Such moral principles, Dworkin argues, provide the grounding for moral and, ultimately, legal rights. Consequently, individual rights are typically subordinated to public policy and judges' desire to act in accord with "the public interest." The tendency of legal positivism to serve majority interest rather than personal rights becomes particularly apparent, Dworkin argues, in "hard cases," i.e., cases in which a specific suit cannot be brought under a clear rule of law, thereby permitting considerable latitude for judicial discretion. Dworkin describes his approach to hard cases by creating a mythical judge, Hercules, and showing how Hercules would resolve hard cases. In each instance Hercules assumes that the parties before him do have legal rights and that it is the judge's responsibility to determine what those rights are and whether or not they have been violated. Presumably, Hercules' personal political convictions play no role in the final determination of the parties' legal rights. That is not to say, however, that Hercules does not exercise any judgment. Judicial judgment focuses on institutional rights. According to Dworkin's theory, difficult cases for which there is no obvious rule of law ought to be decided within the framework of "the rights thesis" which is simply the assertion "that judicial decisions enforce existing political rights" (p. 87). Judgment determines what in fact *are* the relevant political rights. Such judgments frequently require careful moral reasoning.

In the second part of the book, Dworkin attempts to show how his alternative normative theory of judicial decision making applies to a variety of current issues in jurisprudence. Included in this half of the book is the title essay, "Taking Rights Seriously." In this essay the author is concerned with the question of when governmental abridgement of individual moral rights is justified. Dworkin concludes that government can restrict a right only "when some compelling reason is presented, some reason that is consistent with the suppositions on which the original right must be based" (p. 220). In short, his is the traditional liberal position. Dworkin establishes two tests for deciding whether or not a government takes rights seriously. First, such a government must acknowledge the claim that citizens do indeed have a right to break its laws. Second, a government that takes rights seriously must not give priority to the general good when it appears to conflict with the rights of individual citizens.

The right to disobey the law follows directly from what it means to have a moral right against the government. Dworkin refers to this right as a "feature of these rights" and argues that having a moral right against the government means that one is not obligated to obey a law that contradicts that right. To deny the right to disobey the law is in effect to deny the existence of citizens' fundamental moral rights.

For Dworkin the critical distinction leading to this conclusion is that between "having a right" and having the "right to do x, e.g., to break the law." Those who deny the right to disobey the law, the author argues, often confuse these strong and weak meanings of "right." Breaking a particular law may be a right in the first, strong sense even though the community does not believe it to be "right" in the second, weak sense.

For the serious student of jurisprudence *Taking Rights Seriously* should be compulsory reading. It is indeed important reading for anyone interested in the defense of liberal values. In an age when many have written the death certificate for liberalism, Dworkin offers a thoughtful defense based upon the moral principles of human dignity and of political equality. In an age of wire-tap technology, demands for censorship and other potential threats to individual rights, it is not untimely for someone with Dworkin's analytical powers to remind us what it means to take rights seriously.

THOMAS R. CONRAD

Monmouth College, Illinois

Public Administration and Public Policy. Edited by H. George Frederickson and Charles R. Wise. (Lexington, Mass.: Lexington Books, D.C. Heath, 1977. Pp. viii + 232. $14.00.)

Frederickson's and Wise's objective is "to examine the continually evolving relationship between public administration and public policy" (p. vii). More specifically, their focus is on the "organizations tasked with administration of public programs and the *formulation and reformulation of public policy that takes place in the administrative process*" (p. vii, emphasis added). In pursuit of this objective Frederickson and Wise present 15 essays written by students of public administration. The essays are arbitrarily grouped into four sections dealing with human resources (implementation, organizational design, public personnel, management by objectives, and leadership), actors (role theory, federal aid to states, citizen participation, and legislative-bureaucratic relations), decision processes (zero-based budgeting, planning, and management science) and accountability (managing subordinates, productivity, and administrative responsibility).

A book discussing the relationship between public administration and public policy with strong theoretical or empirical analysis would be a major contribution to the study of public policy. Despite the objectives noted in the preface, however, Frederickson and Wise have edited a book with little emphasis on public policy. Instead they have presented a good collection of literature reviews in various areas of public administration. James A. Thurber's chapter on "Legislative-Administrative Relations" is an excellent bibliographic essay. Eugene B. McGregor's discussion of "Future Challenges for Public Personnel Administration" is a concise summary of current public personnel issues. These and other chapters make *Public Administration and Public Policy* essential reading for those preparing for comprehensive exams or for those catching up on changes in the public administration literature.

Despite its value as a reference, the book has some serious limitations that one does not expect from such competent scholars. The book's major weakness is its failure in most chapters to tie the literature being reviewed to public policy. Many chapters present ideas with profound implications for policy studies, but the reader is forced to make these linkages without assistance from the authors. Charles R. Wise's chapter on productivity, for example, discusses effectiveness and efficiency but focuses on the management aspects of these concepts rather than their effect on policy making.

The volume's brevity may be the cause of the authors' failure to trace out the policy implications of the literature they review. Many chapters could easily have been twice as long. C. E. Teasley's perceptive analysis of management by objectives suffers from lack of detail. Charles Levine, who summarizes three major theories of executive leadership in six pages, needs more space to relate these theories to the study of public policy.

Unlike many of the other volumes in the Policy Studies Organization series, this volume presents little original research. Except for David Walker, who analyzes the impact of federal assistance on state and local governments, most authors are content to summarize the findings of others.

A notable disappointment is the editors' failure to focus on the conflicts between authors. Charles Wise, for example, argues that public administration should emphasize productivity analysis to formulate more effective and efficient policy, while Louis Gawthrop in the following chapter argues that increasing analytic capacities of government makes government undemocratic and unaccountable to the public. This interesting question and others languish because neither the authors nor the editors deal with arguments that others present in the volume.

The book is also flawed by a series of stylistic problems. The volume needs an introductory chapter to set the major issues and preview the authors' arguments. The final chapter, presented as a discussion, would do nicely as an introductory chapter with some revision. This change would permit a final chapter noting conflicts, unresolved issues, and future research. The volume also contains several annoying technical errors; references are cited but not included in the bibliography, and the index is incomplete.

The limitations of *Public Administration and Public Policy* do not render the volume useless. As a review of the current state of the art in public administration it is well worth the reader's time. We can only hope, however, that the Policy Studies Organization will present a future volume that relates public administration to public policy.

KENNETH J. MEIER

University of Oklahoma

Methodology and Ideology: Theory and Methods of Social Research, Volume 1. By Johan Galtung. (Atlantic Highlands, N.J.: Humanities Press, 1977. Pp. 271. $12.50, paper.)

In this book, Galtung links the organizational structure of scientific knowledge production both to the larger sociopolitical structures encompassing that activity and to the structural form, methodological basis and substantive content of the knowledge it produces (pp. 13, 40, 214, 244). Scientific methodology epistemologically argues "what constitutes a valid scientific product" (p. 13); similarly, ideology is "a critical analysis of present society, a set of values defining the good society, theories about its viability, and a program for action for its attainment" (p. 70). It follows that "to work with any methodology . . . is a political act of confirmation or negation of the structure in which we, people in general and scientists in particular, live. . . ; one may enact the norms of that methodological grammar . . . correctly or incorrectly. But . . . the choice of a methodology is implicitly the choice of an ideology, including the mystifying, [absolutist], monotheistic ideology that there is but one methodology—the universal one" (p. 40). Or, in other words that a historically informed Chinese radical would easily understand: "[the] anchoring of [scientific] tenability in unlimited extension in time and space [is] a fundamental aspect of the culture that also brought forth modern science; the science that dominates today because it is the science of the dominating [Christian] nations" (p. 53). Have we forgotten the imperialistic theism of Newton's famous poem: "Praise the Lord for he hath spoken/Worlds his mighty voice obeyed:/ Laws, which never shall be broken / For their guidance he hath made" (p. 249).

The greatest challenge in Galtung's book is his transformative methodological synthesis of his own earlier positivist work (e.g., his *Theory and Methods of Social Research,* 1967) with the dialectical epistemology he extracts from Marxist social thought, which produces a globally oriented research methodology based on, but transcending, that thought. This pluralistic, egalitarian, non-Western synthesis is possible only because he ends up advocating a centered, multiparadigmatic, less deductively organized approach to normatively indicative world modeling (chapter 9).

Let me exposit the main arguments in the book, following largely its own chapter 8 on "Positivism and Dialectics." Chapter 1 richly elaborates a typology of social structures and products defined in terms of inequitable and vertical social interactions versus their opposites; and uniform, collectivist, versus diversity, individualist social thematics. Model I (Conservative), Model II (Liberal), Model III (Communal) and Model IV (Pluralist) labels are given for the top-bottom, left-right alternatives thus generated. Marxian stages theory is reformulated in I → II → III → IV terms, ending in a pluralistic model emphasizing self-realization in equality and freedom. Corresponding modes of knowledge production exist: (I) dogmatic, master-revealed, apodictic rationality; (II) positivistic logical empiricism with empirical reality replacing God as the final arbiter and "data as king" (p. 36 ff.); (III) nonaxiomatizable knowledge and the replacement of theory construction about empirical reality by consensus formation about concrete and mutable practical experience; (IV) nonunified, pluralistic science, based on the dialectical rejection of the distinction between the "objective science producer" and the "naive subject as consumer." Kuhn's theory of scientific revolutions is given a most constructive sociological critique (pp. 249–50, n. 28) in terms of his failure to distinguish paradigm shifts within and among these four types of social epistemologies.

In these terms, Galtung's preferred Model IV social science is incompatible with its own negation, a Model I science characterized by verticality and uniformity, centered in the West and held together by hierarchical logical structures (p. 244). Yet Model I science is all too prevalent in authoritarian and democratic forms of Model II (liberal) society dominant today, except in Chinese communes, where (locally) Model III societies may be found (p. 19 ff.). Nonetheless, his easy to read and understand Model III-oriented research program for world order studies (in chapter 9) is an attempt to move social science in the preferred direction.

Galtung provides in chapter 2 a "trilateral" model of social science that synthesizes the positivist, "realistic" emphasis on empirical matching of theory and data sentences with critical comparison of data and values, and a dialectical constructivism that juxtaposes dissonant theory and value sentences to help bring preferred and realizable worlds closer together. "Idealistic" reality creation is at the core of this activist conception which nonetheless (in the image of Model IV) *"make[s] scientists not so very different from people"* (p. 70). Conservative "realism" is given a marvelously idiosyncratic definition: "engage in empiricism to account for the observed world, and then declare that this is the best of all possible worlds" (p. 62), a (Model I) view he finds implicit in Popperian methodological falsificationism (p. 45).

Chapter 3 is an even more dialectical account emphasizing the Marxian epistemological position that *"there are no laws in social science"* (p. 72), within the context of a dialogue between invariance seekers and invariance breakers. The latter emphasis includes liberal critiques of a development-inequity invariance and Marxist counter-critiques that nonviolent transitions to equitable social structures are not possible, but then goes on to suggest that even the "iron laws" of revolutionary master-slave transcendence might themselves be transcended with new modes of nonterritorial, peaceful revolution. Both deviant case analysis and paradigmatic revolutions in ways of defining reality are given a radical restatement as dialectical procedures for realizing more preferred futures.

If chapter 4's quest for diachronic, monothetic social science is more positivistic, it ends with a characteristic dialectical appeal to contextuality. Universalitic "causal" relations are defined in terms of high constancy, regularity, monotonicity and control-variable invariance; they seem to be special cases of process relations, which relax some but not all of these definitional characteristics.

Chapter 4 proposes a radical variant on Merton's liberal functionalism (itself an improvement on Model I-inspired conservative functionalism), that has many Model III and some Model IV elements. The possibility of dysfunctional and eufunctional structures, of compatible and counterproductive structures, means higher order (balanced or unbalanced) contradictions among such contradictions may occur, leading to conservative absorptions of certain functions, evolutionary adaptations in structure or function, and revolutionary systemic alternatives with different necessary or important structures and functions. The self-sufficiency of collectivist self-reliant regions (Model III) or the enhanced autonomy and interaction of genuine global pluralism (Model IV) may lead to a world society unlike those we observe today.

If chapter 6's exploitation-sensitive, yet naturalistic use of organic chemistry to suggest social structural models is reassuringly positivistic, its appeal to synthetic chemistry is engagingly constructivist or even "idealistic." Similarly, chapter 7 starts with a traditional, positivistic account of deductive syllogistic reasoning, transitive in both space and time, but ends up with the assertion "that logical deduction is also an act of faith" (n. 21, p. 266). The characteristically trans-formative suggestion is that porous reality cannot be fit into purely deductive forms even by Model I theories dedicated only to cumulative memory puritanism or "realistic" knowledge generation.

What should we make of Galtung's heretical modifications of the 1950s positivism of Merton, Lazarsfeld, and Coleman? Even if one things Model III specifications need revisions so as to reflect more of what might come after Model II societies and epistemologies, rather than pre-Model I orientations, one must appreciate the genuinely global program of synthetic yet disunifying research reflection and action he sets before us. And one need not treat any survey interviews of randomly selected individuals as fragmentive marginalization (p. 242 ff.). But Galtung has not disowned his Colombian positivism; he has redefined it and gone beyond it, learning from social scientific practices and aspirations in the rest of the world.

HAYWARD R. ALKER, JR.

Massachusetts Institute of Technology

Founding Principles of American Government: Two Hundred Years of Democracy on Trial. Edited by George J. Graham, Jr. and Scarlett G. Graham. (Bloomington: Indiana University Press, 1977. Pp. xviii + 395. $17.50.)

The Moral Foundations of the American Republic. Edited by Robert H. Horwitz. (Charlottesville: University Press of Virginia, 1977. Pp. viii + 245. $15.00, cloth; $2.95, paper.)

The proper founding of a stable political regime is, as various political philosophers have observed, an undertaking requiring extraordinary wisdom regarding political principles as well as an exceptional understanding of human nature. In Rousseau's formulation of this task it was the work of a legislator. In both of the books reviewed here the concern of the authors of the nearly two dozen articles dealing with the founding of the American political system is focused on the principles which the Legislator (founding fathers) sought to put into practice.

The 13 articles in *Founding Principles of American Government* focus as much upon early American institutions as upon the ideological or conceptual atmosphere in which our system of government developed. Many of these articles, we are told in the preface, reflect the influence of Charles S. Hyneman's "Indiana School" on studies of early American thought. Hyneman, who contributed two articles to this volume, sees the years 1775–1800 as crucial to the founding of the American system. In "Republican Government in America: The Idea and its Realization," he notes that by the end of the quarter-century of political discovery and inventiveness following the Declaration of Independence, we had developed a political system containing four distinctive characteristics: "We had government by the people. We had government that was established by law and required to act in accordance with law. The business of governing was dispersed, being vested in many governments rather than in only one or even a few. Within the many governments authority to decide and to act was divided, and wielders of power were put in a position to observe and to check other wielders of power" (p. 7).

In a broad sense, these four features of our governmental system constitute the subject matter for all of the articles in this book. For example, the concept of popular government is further developed by Ross M. Lence in "The American Declaration of Independence" and by Donald S. Lutz in "Popular Consent and Popular Control: 1776–1789." Lence holds that the term "people" found in the Declaration was intended by the authors to mean a majority of the community, and it is the rights of this majority which are proclaimed by the Declaration within the concept of consent. Lutz further develops this theme of majoritarianism in the Declaration and finds that not only does the Constitution not embody the consent theory found in the Declaration, "but in some important respects is contradictory to it" (p. 61). He also provides a table of the frequency of uses, as well as a discussion of the meaning of "consent" in the early state constitutions.

The concept of a government established by law and obligated to act in accordance with law is further developed by Ronald M. Peters, Jr. in "The Written Constitution," and by George J. Graham, Jr. in "The Supreme Court." The concept of federalism and the intention of the framers of the Constitution is discussed by Valerie A. Earle, who maintains that "American federalism has become what it was expected [by the framers] to become" (p. 163). George W. Carey, in his article on the separation of powers, holds that not only did the framers believe that the adoption of this principle was basic to a stable political system, but that "a separation of powers ranks as the most fundamental of our constitutional principles" (p. 98). He notes that the major concern of the framers in employing the separation of powers was to keep the legislative branch of government in check. Other articles discuss the presidency, Congress, political parties, and government and the economy.

Basically, the articles in this volume report the conventional wisdom concerning the political ideas and practices of the founding years of the republic. The various articles are more successful in presenting the thought and institutions of this early period than in assessing these principles and practices after 200 years. For the complexities of political and economic history since 1800 cannot be quickly summarized in a few general paragraphs at the end of each article.

The 10 essays in *The Moral Foundations of the American Republic* approach the work of the framers from a less descriptive but more speculative and philosophical point of view. They are stimulating, well written, and, as Robert H. Horwitz observed in the preface, "they are of such variety that no one can reasonably agree with all of them" (p. viii). Richard Hofstadter's classic essay, "The Founding Fathers: An Age of Realism" is reprinted, perhaps as a foil for Martin Diamond's 'Ethics and Politics: The American Way." Where Hofstadter was critical of the framers of the Constitution for their narrow and rigid view of human nature, Diamond is critical of Hofstadter for succumbing to utopianism in his view of humanity. In Diamond's view there is a distinctive American character, possessed of bourgeois virtues, which accepts acquisitiveness as a virtue while shunning avarice as a vice. Madison, in his political theory, set the direction for the "extended, commercial, democratic republic" (p. 55). For, Diamond observes, "Only the modern commercial spirit flourishing in a complex, modern economy can supply the faction-differentiating division of labor and the great economic diversity that directs the attention of all to the moderating private pursuit of individual economic happiness" (pp. 54–55).

This basically benign and public-spirited view of the founding fathers is sharply challenged by Benjamin R. Barber in "The Compromised Republic: Public Purposelessness in America." In effect, he holds, republican virtue was comprised by the conditions and principles which made for material progress and imperial power. "Thus, for example, it might be said that private property became the surrogate for public norms, self-interest binding men to their public obligations no less surely than shared values once did; that procedural consensus became the surrogate for substantive consensus; that representation replaced participation, as accountability replaced self-government, and autonomy was traded for rights" (pp. 24–25). Consequently, in the new republic "public purposelessness" was placed "at the very core of its value structure" (p. 29).

Joseph Cropsey, in "The United States as Regime and the Sources of the American Way of Life," finds that our present dissatisfactions may be traced to the influence of what he calls "classic modernity" upon our way of life. Specifically, we are under the influence of science, socialism, existentialism, and psychoanalysis. These influences, perhaps more than our political documents, have determined the views and values found in popular consciousness. "Our prospects in our third century appear to depend on the possibility that our moral resources will incline to fortify themselves at the spirited wells of modernity" (p. 101).

Gordon S. Wood, in a most interesting essay, "The Democratization of Mind in the American Revolution," maintains that in setting the machinery of democracy in motion, the founding fathers, as an educated elite, were gravediggers, so to speak, for their own sytle of leadership and role in public life. "As the common man rose to power in the decades following the Revolution, the inevitable consequence was the displacement from power of the uncommon man, the man of ideas. Yet the revolutionary leaders were not merely victims of new circumstances; they helped create the changes that led eventually to their own undoing, to the breakup of the kind of political and intellectual coherence they represented" (p. 103). Robert H. Horwitz discusses Locke's views on education in the context of the query whether a republic based on individual self-interest, and lacking a sense of public virtue, could survive. The Lockean answer, as found in his *Education,* would be no. Locke looked to the educational and political leadership of the gentry, of people of business and of property, and Horwitz inquires whether the training of such gentry would be possible, and if so, whether it would be desirable, today.

Space prohibits more than a mention of Walter Berns' essay on the framers and religious freedom, Wilson Carey McWilliams' essay on the ambivalent character of equality, and Herbert Storing's essay on slavery.

While both of these books will prove useful to students of American political thought and institutions, they serve rather different needs. *The Founding Principles of American Government* furnishes the kind of historical and descriptive analysis which puts the founding of the republic in its proper eighteenth-century setting; *The Moral Foundations of the American Republic* provides the kind of philosophical critique which at once presupposes a knowledge of the events of this early period yet reaches beyond this knowledge to raise basic general questions.

ALAN P. GRIMES

Michigan State University

Science and Technology Policy: Perspectives and Developments. Edited by Joseph Haberer. (Lexington, Mass.: Lexington Books, D.C. Heath and Company, 1977. Pp. ix + 221. $18.00.)

The editor and more than half of the 22 contributors to this collection appear to believe that public participation in making complex technology assessments merits much more attention from scholars and practitioners than it is getting. Such a large number of observations to this effect is remarkable since the editor apparently made no attempt to encourage authors to focus on this or any other specific problem of science and technology policy. The book is divided into three parts, but the categories seem artificial. In 17 selections (several are joint-authored) the essayists go their own way with little effort to concentrate on general foci or to relate their comments to those of other contributors.

But calling attention to organizational style is not necessarily a criticism of the collection. The failure to strain for focus may enhance its value. Here we have political scientists (most numerous among the authors), engineers, planners, policy analysts, operations researchers, an historian of science, a physicist, a chemist, and an economist independently alerting us to policy implications of the ever-increasing complexity of technological society. That so many of these specialists stressed the question of public participation was a welcome surprise. Those "participatory" mechanisms introduced into administrative rulings by legislation such as the Energy Reorganization Act, the National Environmental Policy Act, and the Federal Water Pollution Act have not really been taken seriously by the political science profession as potential significant forces in the American political system. Many of us have tended to view these mechanisms as little more than residuals from the activist mentality of the sixties. Several essays in this volume challenge such a cavalier treatment of public participation.

These calls for greater attention to public participation seem to be based upon one or both of two assertions which surface in several different essays: (1) that scientists and engineers, regardless of their good intentions, must always be viewed as political actors, like all human beings; and (2) that even the best expert forcecasting for the future may be no better than that of an enlightened lay citizenry.

Regarding the "human" characteristics of scientists, the most devastating critique comes from a physicist, Charles Schwartz, who argues that public interest science will have to face squarely questions about possible conflicts of interest. He also believes that "greater insight can be gained by considering a scientist as a political actor operating in the political milieu than by ascribing any common set of characteristics to him based on the fact that he is a scientist" (p. 40).

Dorothy Nelkin, a planning specialist, similarly notes that technology assessment itself is "a political process," and suggests that useful assessment mechanisms may require "adversary of participatory controls" (p. 75). Technical competence and efficiency may not be a sufficient basis for authority in decisions in such areas as public health and safety, she says.

The broader-based questioning of technology forecasting as an activity is led by Edward Wenk Jr. and Thomas J. Kuehn, both engineers and the former a veteran in both White House and congressional science advisory circles. They caution against a "commitment" to rational decision making which, they argue, continues to be an occupational hazard of "policy analysts." Such analysts, they maintain, ignore the limitations arising from the nature of the political process (p. 161). In the most pessimistic contribution in this collection, Wenk and Kuehn disclaim the simple faith of some scientists and engineers that people can control their own fates. Indeed, according to Wenk and Kuehn, it should be apparent to those who view themselves at the helm of the ship of state, "that the helm is but loosely connected to the rudder, even in cases when policy is deliberated [sic] and decided at a high level" (p. 161).

In less metaphoric terms, Nelkin simply contends that most technology assessments are "constrained by methodological difficulties" and quotes others to the effect that forecasting defies chance as if it were dealing with a reality independent of man" (p. 74).

Do some of these essays, then, represent harbingers of a new Luddite mentality among those who concern themselves with government and technology? Not necessarily. Rather they seem to plead for broader assessment processes which would include not only the best minds of our technological community but also those who call themselves humanists and philosophers and even some who are simply citizens. These are not the emotional pleas of the sixties and early seventies. For the most part, they are reasoned presentations which argue that in complex decisions dealing with massive energy systems and similar challenges we should seek broader participation even while risking political paralysis, polarization, and costly delays.

In references to such broader public participation in technology assessment there are touches of nostalgia. For example, Susan G. Hadden, a political scientist, reminds us of Harold Laski's warning nearly 50 years ago (in Fabian Tract No. 235, 1931) of the dangers of citizens abdicating their responsibilities for political choice to experts. In his "democratic paradigm," Laski asserted that experts "tend to neglect all evidence which does not come from those who belong to their own ranks" and are narrow and "lacking in humility" (p. 81). Though they have been active only on the fringes of the current technology assessment "movement," political scientists have enough reason to linger for a moment on Laski's admonitions.

Boyd R. Keenan

University of Illinois, Chicago Circle

Mass Society and Political Conflict: Towards a Reconstruction of Theory. By Sandor Halebsky. (London: Cambridge University Press, 1976. Pp. ix + 309. $19.95.)

Drawing principally from the writings of William Kornhauser and Robert Nisbet, Sandor Halebsky analyzes mass political theory in some detail. This analysis is followed by a sharp and perceptive critique of the theory.

The focus of mass theory is on the anomic nature of the individual in the mass: the rootlessness, ignorance, and confusion—with little if any commitment to democratic values—which lead, under stress, to irrational and disruptive political acts. As perceived by mass theorists, the central function of a pluralistic society is to keep people absorbed in concerns of everyday life—in their work, family, church and the like—to minimize their availability for mobilization by counter-elites. Keeping people thus busy greatly mitigates mass pressure and demands that would otherwise hobble the independence of elites essential to maintaining the system. As underscored by Halebsky, the pluralist model becomes an integral and essential part of mass political thought.

Halebsky persuasively argues that mass theory is basically defective. He marshals considerable evidence to show that it is not unusual that individuals who appear most integrated within groups are those most affected by and supportive of mass movements. He also cites numerous instances in which intermediate groups and established elites are instrumental in mobilizing and legitimating these movements. But more importantly, by emphasizing the social-psychological roots of mass movements, to the exclusion of ideological, structural, and class forces, the gross mass-nonmass concept of society is wholly inadequate to identify a social movement growing out of severe economic and social deprivations, demographic changes, development of class consciousness, or new life styles. Moreover, mass theory refuses to consider that direct political action by a movement may be a rational response by non-elites to the inaccessibility to decision-making arenas. Part of the problem, Halebsky argues, is the failure of mass-pluralist theorists to consider that a genuine political interest may transcend group self-interest and incremental demands. Unable to accommodate broad-gauged class demands within the pluralist's schema, theorists discredit these demands as irrational, springing from rootless and anomic masses. Not only are the mass-pluralist theorists blind to the rational nature of radical protest movements, Halebsky contends, but they are also oblivious to their potentially democratic impact on the political system.

In one of the most interesting chapters of the book, Halebsky reviews the literature on several protest movements, including the American populists, the radical Chilean working-class movement, and the French Communist party, to demonstrate that class movements are often an outgrowth of the rigidity and unresponsiveness to non-elite demands that are both reasonable and tolerant of the needs of other groups in society. While he succeeds in making his point, it would have been strengthened if he had confined himself to an analysis of one or two rather than several movements.

As a whole, the essay is, in the English tradition, scholarly and well argued. It draws upon a widely diverse and extensive theoretical and empirical literature and it is admirably successful in integrating the two. Although the essay is not an original work, it is a provocative and insightful critique of an important branch of contemporary political theory.

PETER BACHRACH

Temple University

Rational Behavior and Bargaining Equilibrium in Games and Social Situations. By John C. Harsanyi. (Cambridge, England: Cambridge University Press, 1977. Pp. x + 314. $29.95.)

One of the great myths afloat in the social sciences today, with the possible exception of economics, is that people are not "rational." If this book does nothing else, it demonstrates that a proper *definition* of rational behavior in a game depends very much on the context—among other things, the information available to players, their ability to make commitments, and their communication possibilities—as well as the players' goals that are described by their utility functions. Harsanyi does not attempt to answer the question whether players in real-life games are in fact rational, but *if* they are, he shows what we should observe empirically in the context in which they interact. Thus, this book goes a long way toward setting straight *what to look for* in game situations to predict their outcomes, which are assumed to depend not only on one's own choices, and perhaps chance, but also on the choices made by other players.

Harsanyi restricts his analysis largely to "classical" games in which (1) players have complete information, (2) players either can or cannot make enforceable agreements before play of the game, and (3) players cannot make commitments to strategies, which define sequences of choices, after play has commenced. This last restriction limits the class of game studied to those that can be adequately represented in normal, or matrix, form, which is the form most commonly discussed in the political science literature.

Harsanyi's singular contribution is to show that, given certain "rationality postulates" that characterize the behavior of players in all classical games, one and only one outcome is possible. In other words, Harsanyi's theory, which is really a theory of rational behavior in game situations, predicts a unique outcome in all classical games.

This is a very strong result and, at least superficially, not in the spirit of classical game theory. With the exception of the minimax result in two-person, zero-sum game theory, and a very few solutions concepts in n-person game theory (like the Shapley value) that yield determinate payoffs, most solution concepts (like von Neumann and Morgenstern's stable set) are not in general determinate. In fact, the set of possible solutions is often embarrassingly large, making the empirical verification of solutions–at least outside the laboratory–a difficult task.

Harsanyi, of course, pays a price in constructing a general, but completely deterministic, theory. His rationality postulates have to be supplemented by elaborate conditions (i.e., detailed factual information in an empirical situation) to yield a specific prediction. This makes the theory complex and difficult to apply.

The main difficulty stems from the fact that players who have only imperfect information about moves during play of the game must calculate on the basis of subjective probabilities, even if they have full knowledge about each other's payoffs and strategies at the beginning of the game (i.e., complete information). The principle Harsanyi invokes, within a Bayesian framework, is that of mutual expectation of rationality, whereby all players in a game are assumed to act on the expectation of rational behavior by all other players. How players decide on expectations, and what this implies for the choice of subjective probabilities, is the subject of the theory. Principally, the theory concerns how players bargain, and what equilibrium, or stable, outcomes result as a function of payoff-risk-dominance relations and the communication-commitment process. There is, in addition, a provocative discussion of ethics and its relationship to rational-choice models.

Harsanyi treats many interesting problems, fallacies, and paradoxes (my favorites are the blackmailer's fallacy and the joint-bargaining paradox) in his theory, but one problem he assiduously avoids is what I would call the computation problem. Solutions to cooperative games in his theory involve solving complicated systems of equations, which Harsanyi almost off-handedly mentions can be solved iteratively (p. 176). How this is done is not spelled out.

The computation problem, for example, plagues Harsanyi's modification of the Shapley value to take account of threats, based on a modified characteristic-function representation. Consequently, it is difficult to assess the quantitative implications of Harsanyi's modification, which is not even illustrated by examples.

Some distinctions in Harsanyi's theory seem to define away the interesting and difficult problems. For example, the distinction between cooperative and noncooperative games, which is not the usual one, has an immediate consequence that the cooperative and noncooperative outcomes in prisoners' dilemma are the unique solutions in their respective classes of games. This by-passes the important question of what conditions lead to movement from one kind of solution to the other. Clearly, future work is needed to build an adequate dynamic theory to explain such transformations.

Despite these criticisms, it is hard not to marvel at the theoretical structure Harsanyi has built. Harsanyi says it took him many years, and his own thinking has been changing. This is not surprising: behavior that game theory deals with is rich, relationships among players are elaborate. One does not so much "learn" game theory as live with it, absorbing and internalizing it in an osmosis-like process.

Reading Harsanyi is an intellectual experience *par excellence*, even when one disagrees with him. The questions he asks are central, in my opinion, to the construction of a general theory of politics, though for the political scientist, certainly, the mathematical details of sorting through various cases can be quite overwhelming. Even so, for political scientists growing tired of an uninspired diet of traditional and behavioral political science, and desiring a serious intellectual challenge, I would recommend this book.

STEVEN J. BRAMS

New York University

Rational Choice and Social Exchange: A Critique of Exchange Theory. By Anthony Heath. (Cambridge: Cambridge University Press, 1976. Pp. iii + 184. $16.95, cloth; $5.95, paper.)

Exchange theory is a theory of social interaction and power with a wide range of potential applications to politics. Anthony Heath is the first scholar to write on exchange theory from the perspective both of an economist and of a sociologist. He brings to his subject a clear eye for logical tangles, as well as considerable sensitivity to the range of possible applications of exchange theory. These attributes place him ahead of most exchange theorists, whose interests and competencies are more limited.

Yet this book does not offer a full critique of exchange theory. Heath simply ignores too many recent contributions to the field. He does not discuss Curry and Wade, Sidney Waldman, Ilchman and Uphoff, Andrew Scott, or Robert Salisbury at all, though all these writers have either offered their own versions of exchange theory or used exchange theory in their work. He does, however, discuss three writers on political economy who do not make much explicit use of exchange theory: Olson, Riker, and Downs. Although political economy in their hands incorporates certain assumptions about social exchange, none of these three writers has exchange theory uppermost in his concerns; and, unless one wishes to argue that theories of rational choice and political exchange are *coterminous* (something Heath would deny), it is hard to understand the strategy here. Suffice it to say that the coverage of this book is highly idiosyncratic.

Perhaps the central issue which divides exchange theorists is the question of the compatibility of economic, "rational man" assumptions with sociological analyses of the norms governing exchange, most of which norms have a base in custom and traditions which seem, superficially at least, to be inexplicable in economic terms. Heath's chief accomplishment in this book is to suggest that the sociological and economic sides of exchange theory are not really antagonists, but partners in a joint enterprise. This he demonstrates through often brilliant theoretical analyses as well as compelling illustrations. However, Heath can make this argument only after first demonstrating the incapacity of either of these approaches to do without the other.

Heath agrees that there is much the economic approach to exchange can do effectively by itself. For example, he argues that the economic approach can offer the concept of price as a way of avoiding the knotty problem of measuring political power. In addition, the economic approach can yield unexpected insights, such as that drops in the price of one product or service will yield increased purchases not only of that service or product, but also of alternatives. This process of demand generation could help explain the dynamics of certain kinds of economic expansion. Finally, Heath outlines a method by which utility can be ordered into equal scalor intervals, thus permitting great theoretical precision and the possibility of experimental verification of exchange postulates in politics.

But, despite these promising developments, Heath argues that the economic approach is limited. Economic theories of political exchange apply best to cases where known probabilities can be assigned to a number of events. But these situations cover only a portion of relevant political actions. Moreover, the economic approach remains premised upon the belief that exchange is voluntary, but, of course, coerced exchange (e.g., taxes) often occurs. Finally, too often the economic approach takes as a given the existing social and normative constraints on bargaining, and asks the limited question: "Given these constraints, what is it rational for political actors to do, and what rate of exchange will result?" The problem of the formation of social structural and normative constraints upon exchange stands at the center of political analysis, and some would say that its avoidance of this problem makes the economic approach into a relatively trival theory.

By contrast, *social* exchange theory, as exemplified mainly by Blau and Homans, is intended to broaden the exchange approach to take account of the formation and influence of norms and custom. But Heath argues that so far this effort has not been fruitful. The major problem is that social exchange theorists fail to make important distinctions between expectations and norms. The fact is that norms only provide expectations which guide behavior, and that often behavior deviates from these normatively based expectations. Therefore, behavior is not a direct reflection on the formation or current status of norms. But, in a clever stroke, Heath argues that the necessary distinction between operational expectations and moral norms helps explain political change better than exchange theory has, up to this point in its evolution, succeeded in doing. This insight is the major original contribution of Heath's book.

Pursuant to this insight is Heath's effort to reconcile the economic and sociological approaches. He observes, "The existence of norms does not make the rational choice approach to exchange redundant. At most it means that the alternatives to exchange are sanctions from others or guilt feelings from oneself. These alternatives, however, are not in principle different from any others, although they may be rather harder to establish empirically. Blame and guilt can be weighted in the balance like anything else, and even the most saintly can be tempted to deviance if the gains are sufficiently rewarding" (p. 105). Heath therefore investigates the question of when it is reasonable for people to institute norms, conform to norms, enforce norms, or dispense with norms entirely. These brief sections constitute the most original parts of the book, and one regrets that they are not more fully developed.

There are flaws in this book other than its erratic scope. The book is difficult to read and loosely organized. True, it contains fascinating applications of exchange theory to such problems as bystander intervention in crisis situations, fertility rates, and even cross-cousin marriage in traditional societies, but, ingenious as these examples are, to the political scientist they are more arcane than useful. Despite these problems, however, Heath's book makes up in sophistication and insight what it lacks in comprehensiveness and synthesis.

RICHARD M. MERELMAN

University of Wisconsin, Madison

Decision Making: A Psychological Analysis of Conflict, Choice, and Commitment. By Irving L. Janis and Leon Mann. (New York: Free Press, 1977. Pp. xx + 488. $15.95.)

During the past decade, the boundaries separating the various academic disciplines have eroded to the point where the overlap may, in some cases, match or exceed the areas of disciplinary autonomy. The effects on scholarly inquiry are potentially profound. For political science, this trend means that the expanded repertoire of theoretical and analytical tools compels a continuous reexamination of approaches to the study of politics.

This volume by Irving Janis and Leon Mann illustrates the benefits of increased cross-fertilization between social psychology and political science in research into decision making. The authors piece together a wealth of findings and theoretical foci and propose a general conflict theory of decision making applicable to individuals acting alone, in small groups, and in organizational settings. Much of their data are derived from studies seemingly unrelated to politics (e.g., decisions to change jobs, to quit smoking, to heed warnings). These studies appear less remote, however, when they are supplemented by political case studies (e.g., Pearl Harbor, the Watergate scandal, desegregation). Their theoretical foundations owe much to (and challenge portions of) cognitive psychology, consistency theory, attribution theory and group dynamics, although the synthesis is original and carefully grafted.

It is interesting to note that while political science has not shied away from an analysis of decision making, most of this research has either paid homage to rational/analytical models of decisions or has riveted attention on decision outcomes at the expense of process variables. Janis and Mann, however, are almost exclusively concerned with process variables and how decision outcomes are traceable to a host of antecedent variables, most of which are psychological and social-psychological. For them, the individual is a "reluctant decision maker" who struggles and searches for ways to cope with the multitude of personal conflicts posed by the problems to be faced.

Janis and Mann are primarily interested in identifying the interplay of factors which influence how people make choices when confronted with decisional conflict in "consequential situations." Consequential situations are those in which the individual has an important stake in the outcome. Hence, routine situations, or those having low value priority, or even those decisions derived from simulations or games, are excluded by the authors because they lack consequentiality. In political research, this delineation would make their theory most applicable to studies of elite behavior in crisis decision making particularly to foreign policy decision making where research on crisis-related individual behavior is most extensively developed.

More importantly, Janis and Mann tackle the elusive topic of what determines quality decisions by defining quality in terms of the process or procedures used to select a course of action. Good decisions are those characterized by "vigilant information processing" which occurs when a person adheres to a rigorous seven-fold checklist involving systematic search, appraisal, and contingency planning. Deviation from these criteria leads to defective information processing exemplified by various forms of "defensive avoidance" such as buck-passing, procrastination, or pre- and post-bolstering as well as over-reaction in the form of "hypervigilance." These and other maladaptive coping mechanisms are likely to result in decision error or post-decision regret.

The authors then explore the conditions conducive to or interfering with vigilant information processing. Of particular interest are the various social-psychological forces which membership in small groups or larger collectivities impose on individual behavior. Examples include the "psychology of coercive demands" and the "illusion of no choice" so well documented in Hannah Arendt's work on Adolf Eichmann or in more recent illustrations of individual compliance to malignant authority in the protracted Watergate scandals. Another example is Janis' well-known case study of interpersonal and group-based pressures to conform between Admiral Kimmel and his staff on the eve of the attack on Pearl Harbor. Such application to political cases is likely to spark interest among political scientists whether or not they believe that most of the variance in political behavior can be explained by reference to the thought processes of individual decision makers.

Building upon their research findings, Janis and Mann propose a number of practical and practiced intervention techniques designed to reduce faulty information processing. Each technique is intended to improve the quality of individual decision making by promoting increased information search, challenging outdated assumptions, stimulating awareness about self-bias, preparing for post-decision reverses, or by devising ways to minimize "groupthink" among decision makers. The techniques–e.g., decision counseling, outcome psychodramas, emotional inoculation against setbacks–unfortunately seem better suited to the business world of executives (and others) than to the sensitive world of politics, if only because the former is more open and experimental than the latter. Except for this obvious difference, the proposed prescriptions would seem to have applicability across occupational groupings and substantive problem areas.

To synthesize their findings, the authors wisely turned to an information-processing framework which scholars increasingly have found to be an attractive paradigmatic alternative to the maze of theories utilized in research on decision making. The next step for those who study decision making is to integrate these findings into a bureaucratic and organizational research and other macro-level theories of political behavior.

ANDREW K. SEMMEL

University of Cincinnati

Karl Korsch: Revolutionary Theory. Edited by Douglas Kellner. (Austin and London: University of Texas Press, 1977. Pp. vii + 299. $14.95.)

This book attempts to fulfill a need for an English edition of the selected occasional writings of Karl Korsch (1886–1961). His reputation as a minor but stimulating thinker at the fringe of the critical school of Marxism has been growing over the last two decades and this compilation gives us some chance of assessing the quality of his work–or rather would have given us some chance had it been properly edited and compiled. In place of bread we have been given stones.

It may seem churlish to begin a review with several paragraphs criticizing the editorial defects of a work, but in this case we must. This is a classic case of a non-book. Its title and size would indicate to the unwary that this volume contains a large amount of Korsch's writings. However, only 152 of 306 pages are devoted to Korsch's own work, the rest being editorial. Nor is it entirely material unknown in English: much has been available elsewhere, though with difficulty.

In stating that this is a non-book I refer to more than the disproportion between text and introductory material. There is material for a book here but not the book itself. The introduction, for example, provides much useful information on Korsch's life, theoretical development and political involvements, and also corrects some misconceptions as to his role in the German Communist party in the early 1920s. But it is repetitive, diffusely argued and should have been reduced to less than half its length. Furthermore, press editing is notable by its absence. There are errors of both grammar and syntax. Nor should Kellner have been allowed to repeat his introductory material at the start of each section of texts following page 113. The translations are often awkward and one must reread far too many sentences to fathom their meaning. Nor does one need the German text to recognize an error in the following: "as long as the state exists, one has to demand the evil of analphabetism be eliminated!" (p. 217). It is difficult to find the dates and origins of the pieces selected without going back to the introductions, and the index lacks subject headings. Russian names are persistently Germanized and confused: there are three different spellings for Mikhailovski (pp. 155, 160, 275) and two for Zinoviev. More importantly, sources have not been provided to those quotations and references unidentified by Korsch nor have his footnotes been modernized by reference to modern, easily accessible editions.

These remarks should not be construed as an evasion of the task of seriously considering the book's content and argument. The duty of a writer or editor of a scholarly work is to provide material that would enable us to consider the argument and *go beyond it.* Kellner's introduction makes a case for considering Korsch as an exponent of a cogent theory of counter-revolution (pp. 86 ff., 232–33, 236 ff.), as a critic of the "Blanquist-Jacobian [sic] features" of Marxism (pp. 81, 167–76), as an advocate of the importance of the Third World and as a stimulating if not always cogent critic of the political practice of Marxist and Marxist-Leninist parties. The excess of editorial material prevents the translation of that extra amount of Korsch which might have enabled us to justify these claims. Further, Kellner seems to share Korsch's views without being able to assess how correct they might be in relation to Marxism. It is understandable that Korsch in the 1930s would detect elements of Jacobinism and Blanquism in Marxism, but in the 1970s it is Kellner's scholarly duty to inform us that the critical literature of the last decade runs against that view. In brief, Korsch presented theses for research—but in the intervening years much of that research has been done.

The best that can be said for this book is that is presents material not easily or otherwise available. Regrettably, it might prevent a better edition from appearing.

ROBERT A. FENN

University of Toronto

Hegel's Retreat from Eleusis: Studies in Political Thought. By George Armstrong Kelly. Princeton University Press, 1978. Pp. ix + 259. $16.50.)

The postwar Renaissance in English-language studies of Hegel's political philosophy, to which George Kelly's *Idealism, Politics and History: Sources of Hegelian Thought* (1969) was so important a contribution, is now enriched by a new offering from Kelly—or rather by a book which is partly new, partly a retrieval of some splendid journal articles finally finding permanence.

This division between the new and the old reflects a distinction drawn by Kelly himself, who tells us that parts of his book involve an "archaeological" clarification of "Hegel's place in the European thought of his tradition and period," while other parts attempt to carry Hegel forward into the present, particularly by asking whether the Hegelian notion of "the neutral state" is living, dead, or moribund. Since, for Kelly, "true humanism means both recovery and replenishment," "consequent" writers on Hegel try both to understand and to "use" him: "the 'renovators' spread the sail, the 'antiquarians' hold the tiller."

The opening chapter, "Politics and Philosophy in Hegel," is a (successful) effort to "place" the state in Hegel's entire scheme of thought. Hegel was "a state worshiper," Kelly urges, not because he "revelled in the state's irrational emotional residues" but because "he saw it as the organizing principle which, through the institutional civilizing of the 'situated' human being and the protection of his higher values from disruptive disorder, made the creation of culture and philosophy possible in the modern age." Faithfully mirroring the *Preface* to the *Philosophy of Right,* Kelly reminds us that, on the Hegelian view, "although the philosophical understanding contains a critical element, it has also a justificatory duty: it does not abolish what is, in a vain leap toward some imagined perfection ... rather it familiarizes the mind of man with the conditions discovered in the actual world." But Kelly takes care to balance Hegelian political "realism" (some would say quietism) by quoting—in his illuminating third chapter, "Schiller and Hegel"—Hegel's insistence that "when the existing world of freedom has become faithless to the will of better men, that will fails to find itself in the duties there recognized and must try to find in the ideal world of the inner life alone the harmony which actuality has lost." After all, as Kelly's fifth chapter, "The Neutral State," makes clear, the "most striking element" of Hegel's political theory is that he, like Rousseau and Kant, "takes the subjective will to be a cornerstone of modern government" a "will" on which, however, Hegel "imposes a higher 'Hellenic' or *sittlich* [ethical] goal of public virtue and public service," so that in the end the Hegelian state "must be collectively educative and secure to the freedom of the subject, at the same time; both Platonic and Kantian in the 'moments' of its dialectic."

That the Hegelian dialectic has more to do with Plato and Kant than with Marx is a point brilliantly made by Kelly in his second chapter, "Lordship and Bondage," which criticizes Alexandre Kojève's celebrated quasi-Marxian "reading" of Hegel as a theorist of "mastery and slavery." In his *Idealism* Kelly had complained that Kojève makes the master-servant "tableau" of the *Phenomenology* into a "synoptic clue to a whole philosophy"; in his new book he argues that while "every student of Hegel is deeply enriched by Kojève," nonetheless in Kojève's reading "the master-slave relationship is made an unqualified device for clarifying the progress of human history." The notion that "the future belongs to the slave" [e.g., the "proletarian"], Kelly urges, is an "unwarranted romanticized refraction of Hegel's thought." Though Kojève's "original exegesis of Hegelian themes" is "profound," a "Marxian" *Phenomenology* "does not make very good sense." Such a reading "ignores the depth and passion of Hegel's Greek attachments; it ignores, too . . . his struggle with the Kantian split vision. These are the two combatants wrestling on the soil of Christian Europe for the possession of Hegel's own ego."

Relaxing his grip on the "antiquarian" tiller (which is never wholly relinquished), Kelly turns his attention to "renovation" of Hegelian notions in a 'final cluster of four essays" which "grapples, in somewhat new fashion, with Hegel's theory of the state, its historical and structural foundations, its demolition by a following generation, and, finally, its relevance." He begins, in a chapter called "The Modern State," by declaring unequivocally that "the state is a political term, and politics is to be viewed autonomously." We are not, he says, "likely to achieve heaven on this earth"; therefore "some semblance of laws and coercive power will persist as long as there is a society of men."

But there are states and states, and what Kelly wants reconsidered is Hegel's idea of a "neutral" state: "a state that is neither religion on earth, nor the elastic plaything of factions, nor the captive of a fictive ideology that defeats the human experiment." Is it, Kelly asks, "simply delusion and ideology to conceive of or aspire to the rule of wisdom and impartiality in politics, or must all government simply be either class-dominant or the temporary resultant of the complex play of social forces?" Hegel's answer was "no," and it is Kelly's answer as well.

Kelly admits, of course, that the notion of an impartial rule by a truly "neutral" state has been "hideously tarnished": "gravediggers" of various stripes have shattered the Hegelian *syntagma.* On one side of Hegel "liberalism, in its most superficial moments, had wanted to liberate man for the widest practice of his willful caprice consistent with the endurance of society." On another side "Marxism, in its most generous manifestations, despised the result" to which liberalism led: "Mahogonny." Marxism, on Kelly's view, "wished to combine men in a common society of sharing and respect, but it cared scarcely for man's heritage." Hegel alone, according to Kelly, attempted something harder than anything envisaged by either liberalism or Marxism: "liberating men by binding them to their heritage; by reminding them that nothing valuable to civilization [e.g., the state] need be lost . . . *and that freedom, to be meaningful,* required the discipline of memory." (This view, which is Kelly's as much as Hegel's, is applied with interesting results to Rawls, Nozick and Hayek.)

Nothing, however, could provide a finer overview of Kelly's enterprise, both antiquarian and renovating, than the magnificent opening paragraph of *Hegel's Retreat from Eleusis.* "The title of this book was suggested to me," Kelly says, "by rereading the youthful poem written by Hegel to his friend Holderlin in August 1796. It is a poem bristling with both ideological intransigence and Romantic trust and friendship. In it Hegel declares: 'to live for free truth only; but for a peace with statutory law that regulates opinion and men's feelings—never, never to consent to that!' We have all known gifted young men who said 'never, never.' But the instances are rare where the repudiation of that 'never' produces such fertile and honest results. As Hegel retreated from his Romantic world of Eleusis, he discovered and clarified a world at the service of human reason. But as he went forth into midday and afternoon to bequeath a majestic, but cooler, vision of his culture, he did not totally lose his attachments to that point of departure. The free truth was not forsaken; but Hegel also sought to achieve peace—the peace of the intellect and the peace of the city." This passage, so brief and yet so fully manifesting an Augustan poise and balance that a Dr. Johnson might have admired, reminds me of what Richard Strauss once said of the few bars which lead into the Masked Trio of *Don Giovanni*: that he would gladly give everything he had written to have produced those few measures.

PATRICK RILEY

University of Wisconsin, Madison

Politics and Ideology in Marxist Theory: Capitalism—Fascism—Populism. By Ernesto Laclau. (Atlantic Highlands, N.J.: Humanities Press, 1977. Pp. 203. $12.00.)

Since the end of the Second World War, many young Marxist scholars in Europe and Latin America, and more recently in the United

States, have sought to reconceptualize the conventional wisdoms that were the legacy of the Comintern and of the cold war. Through these efforts, there has developed a new vitality in Marxist scholarship which has come to challenge, in part, not only "official" Marxism but also much of contemporary "bourgeois" social science. This vitality is demonstrated clearly in Ernesto Laclau's *Politics and Ideology in Marxist Theory: Capitalism–Fascism–Populism,* an important book which merits serious attention from social scientists of any theoretical persuasion.

In four surprisingly well-integrated essays, Laclau argues strongly and persuasively against the "taxonomic" and class reductions found so often in contemporary Marxist scholarship and against the facile paradigms of bourgeois social scientists. "Theoretical practice has been greatly hindered by the connotative articulation of concepts at the level of common sense discourse and their rationalist articulation into essential paradigms" (p. 10). Instead he calls for "an increasingly theoretical formalization of Marxist categories" (p. 12) which, essentially, is the goal of the essays collected here.

All of the essays have a similar form. In each, a major problem of Marxist theory is introduced–in turn Laclau focuses on "capitalist" development in Latin America; on the bourgeois state; on the nature of fascism; and on the phenomenon of populism–through a critical review of one or more recent studies. Major concepts are scrutinized for their theoretical worth. And in every essay he provides, through an often eclectic use of others' concepts, an original analysis of the problems discussed as well as a solid, though admittedly tentative, theoretical alternative to explanations currently in vogue.

His essay in fascism, which appears for the first time in this volume, is particularly interesting. In it Laclau attempts to revive a theoretical discussion of the nature of fascism, particularly its class origins, a discussion which long had been stilled. Contemporary Marxist analysis, still heavily influenced by Comintern interpretation, has eliminated "the complexity of fascism and [has] reduced it to a single contradiction: that existing between monopoly capital and the rest of society" (p. 88). Although the essay is too complex to review in its entirety, Laclau's sophisticated use of Althusser's notion of "interpellations" to reopen theoretical debate is indicative of his fresh approach.

Laclau dissents strenuously from the "process ot pronouncing the class belonging to elements of concrete ideologies" (p. 97) and holds that Marxist scholars, in their reflexive class reductionism, have overlooked a critical insight contained in the work of Hannah Arendt. "It was not interpellations as a class but interpellations as 'people' which dominated fascist political discourse" (p. 136). "Fascism, far from being the typical ideological expression of the most conservative and reactionary sectors of the dominant class was, on the contrary, one of the possible ways of articulation of popular-democratic interpellations into political discourse" (p. 111).

That is, Marxist analysis and political practice did not recognize that classes struggle simultaneously at two ideological levels: as a class and as the "people." For the petty bourgeoisie, whose conduct is critical to an understanding of fascism, "identity as the people plays a much more important role than identity as a class" (p. 114). But the ideology of the petty bourgeoisie is too diffuse to "organize its own discourse and can only exist within the ideological discourse of the bourgeoisie or the proletariat" (p. 114). The working class, because of the theoretical reductionism of its leadership and the political line imposed on it by the Comintern, erected a class barrier and thereby ceded the "arena of popular-democratic struggle" to the "monopoly fraction" (p. 124). The possibility of revolutionary confrontation arrives, however, when the ideology of the working class is able to present itself as the heir to national tradition and when the "anti-capitalist" struggle becomes the culmination of the democratic struggles of the entire society over the "dominant bloc." It has been overlooked, and here is one of Laclau's insights, that while democratic struggle is dominated always by class struggle, "popular democratic interpellations" do not have a "necessary class belonging." This Tito, Mao, and Berlinguer understood.

During the rise of fascism, the working class failed to articulate itself as a "hegemonic popular alternative" to the dominated classes as a whole. The working class isolated itself ideologically and politically while the "monopoly fraction" interpellated the middle classes en route to absorbing and neutralizing them.

For Laclau the contemporary lessons of fascism are clear. Unless the working class can avoid the dual perils of class sectarianism and social-democratic opportunism, it will never be able to develop "hegemony over the remaining popular sectors" (p. 141). The need to understand these perils is particularly critical today when the European working class is attempting to widen its influence over the middle classes. A "theory of the specific autonomy of popular democratic interpellations" (p. 142) now becomes a necessity.

Marxist or not, the reader of Laclau's work stands to benefit. Laclau's rejection of the sterile formalism of past Marxist theory and practice is refreshing as is his rigorous attempt to rework earlier Marxist categories of analysis (which unfortunately could only be hinted at here). One minor criticism: an extensive bibliography and a detailed index would have been helpful.

RICHARD J. BORIS

York College,
City University of New York

Rousseau's Political Philosophy: An Exposition and Interpretation. By Ramon M. Lemos. (Athens: University of Georgia Press, 1977. Pp. x + 252. $14.50.)

Utterly eschewing all biographical and historical perspectives, Ramon Lemos asks simply how far Rousseau's political philosophy is true, how far it is worth anything. Thus his constant method is to take up this or that proposition in Rousseau ("the general will is always right," "man is born free, and everywhere he is in chains") and to ask what sufficient reason we have for accepting such a proposition as correct. It turns out that for Lemos Rousseau is correct about politics, and accordingly two recurring judgments in Lemos' treatment are: "this certainly seems to be a reasonable position" and "this is true for the following reasons." Since Lemos always offers his own reasons for finding Rousseau's reasons reasonable, his argument is not a mere stringing together of *obiter dicta;* and he almost always succeeds in supplying Rousseau with a sense that looks like common sense. It is not surprising, then, that Lemos should claim that Rousseau's "republicanism" rests upon the perfectly correct conviction that "so long as men are subject to laws prescribed to them by others, and do not accept the responsibility of freely imposing laws of justice and equality upon themselves, they remain in tutelary bondage to their legislators and fail to develop fully their capacities as free and responsible moral agents" (p. 131). This is obviously a possible view, and one which allows Lemos to conclude that "the political philosophy of Rousseau is a greater achievement than that of any other thinker in the natural right, natural law, social contract tradition" (p. 252). Indeed, the *only* aspect of Rousseau's thought which Lemos finds unacceptable is his treatment of Christianity as something incongruent with republicanism: "perhaps the most charitable thing that can be said about it is that it leaves something to be desired" (p. 212).

The idea that Rousseau's treatment of Christianity needs to be viewed "charitably," while the rest of his philosophy is "reasonable" and "a great achievement," suggests that Lemos takes "Christianity" to be central in assessing what a philosophy is worth; and for him Rousseau's "achievement" is "great" *despite* his "mistaken and misdirected" view that good Christians make poor citizens (a view which "adds nothing of importance" to Machiavelli's complaints). At first, actually, one thinks that Lemos must be some sort of Kantian, since he insists on "respect for the moral law" as a notion valued as much by Rousseau as by Kant; but in the end it turns out that for Lemos both Rousseau and Kant are "Christian individualists" (despite Rousseau's "mistaken" treatment of Christian other-worldliness as antipolitical), and that *being Christian* is the ultimate palm to be awarded.

Now being "Christian," according to Lemos, does not mean anything so particular as believing in Aquinas or in natural law. There is "at least one sense," he says, in which "Rousseau is a profoundly Christian thinker," inasmuch as "his position that morality and justice are more important than culture and civilization is the position of Christianity" (p. 4). Surely, though, this is an excessively latitudinarian reading of "the position of Christianity": on this logic, anyone who fails to defend *ars gratia artis* is a "Christian," and this makes (say) Plato, who was so hard on itinerant entertainers in the *Laws,* a "Christian" even without the transforming efforts of Plotinus and St. Augustine. (Later, when Lemos distinguishes "Christianity" in particular from anti-aesthetic moralism in general, he stresses the notion of "charity": Christian morality, he urges, "is summed up succinctly in these words from St. Paul. 'Though I speak with the tongues of men and of angels, and have not charity, I am become as sounding brass or a tinkling cymbal.'" The Christian "position," he goes on, is "summed up also in these words from Kant, Rousseau's disciple. . . . 'Nothing in the world can . . . be called good without qualification, except a *good will.'* " As for Rousseau himself, Lemos claims, "we do not . . . do excessive violence" to him if we interpret him "as addressing the words of St. Paul not only to *les philosophes* of the Age of Reason, but also to intellectuals of every age" [pp. 4–5].)

While it is arguable that Rousseau's and Kant's stress on "will" ("general will," "good will") is *somehow* "Christian," the Pauline idea of "charity" is a slightly different matter: here one can complain that if Lemos' original definition of Christianity was too general—such that every major philosopher save Nietzsche becomes a "Christian"—the final definition (stressing charity) is too particular to accommodate Rousseau and Kant, unless one calls Rousseauistic "compassion" charity (as Rousseau himself did not) and unless one calls Kantian "respect for persons as ends in themselves" charity (as Kant himself certainly did not).

Lemos' insistence on charity does something rather peculiar to his interpretation of the "general will," which he calls "one of the central notions in [Rousseau's] political philosophy." Now one characteristic of "charity" is surely universality, inasmuch as all Christians ought to treat all people charitably. Since Lemos reads Rousseau as "addressing the words of St. Paul" to the Age of Reason, he cannot believe that a truly universal "general will" could be anything other than a will for the common good of the whole *genre humain*: "the only will that is absolutely general . . . is that of the entire human race" (p. 141). While Lemos finds some support for this universalism in Rousseau's *Political Economy,* his insistence on "Christian" universality leads to not even mentioning the important work in which Rousseau denies flatly that there can be a "general will" of the whole "human race." In the *Première Version* of *The Social Contract,* Rousseau takes up Diderot's argument for a "universal" general will, but only to repudiate it: "There is no natural and general society whatever between men . . . it is certain that the word *human race* presents only a purely collective idea to the mind, which supposes no real union between the individuals who comprise it." And Rousseau ends by complaining of "pretended cosmopolites" who, "justifying their love of country through their love of the human race, boast of loving everyone, to have the right of loving no one." Admittedly, Rousseau is not always thinking of so particular a general will (of Sparta, of Rome, of Geneva, of Poland); but this particularity is general enough that one can suspect Lemos' insistence on charity, on a universal "general will."

If insistence on universal charity causes much of Lemos' problem with the "general will," there are still excellent things in his book. Thus there is a fine section in which Lemos asks what Rousseau can possibly mean by saying that people are "born free" but are now "in chains"; there is an interesting passage dealing with the difference between self-love and egotism; and there is a fine contrast between Rousseau and Marx which succeeds in characterizing the exact nature of Rousseau's radicalism. These sections are achievements which stand in no need of charity; Lemos is at his (considerable) best when he is farthest removed from sounding brass and tinkling cymbals, and finds himself assessing the reasonability of Rousseau's reasons.

PATRICK RILEY

University of Wisconsin, Madison

The Life and Times of Liberal Democracy. By C. B. Macpherson. (New York: Oxford University Press, 1977. Pp. 120. $6.95, cloth; $2.50, paper.)

The power of corporate capitalism may be disconcerting, its great ideological servants from James Mill to Joseph Schumpeter may have succeeded in making it appear compatible with liberal-democratic commitments, and the impersonal forces of an uncontrolled technology may ominously loom in the background. Yet light remains; redemption and progress are not impossible. With a cool head, sensible discourse, and a ready command of the language, C. B. Macpherson shows his faith that an unenlightened opposition may yet be engaged. Untouched by irony, he accepts the dominant definition of good sense and makes judgments based on an analysis that is precise and scholarly. Whatever pain and rage move him as he reflects on the contemporary human condition, no trace of it is visible in his meticulously formulated, briskly argued, consistently *reasonable* disquisition. With Macpherson dispassionately in charge, we are all promising graduate students.

We learn first that because Rousseau's and Jefferson's vision had been fitted to a classless society, currently relevant thought about liberal democracy must begin with more recent models. The first of these models, we are told, is the one constructed by James Mill and Bentham. Lamentably, it was designed to do no more than improve the performance of the rich as governors and protect the citizenry against oppressive governments. Leaving a competitive market and class divisions undisturbed, it deflected all enthusiasm for equality (pp. 42–43).

The increasingly obvious exploitative character of a class-divided society and the restlessness of the working class required the amendment of the original model. John Stuart Mill responded by stipulating the possibility of the further development of mankind toward an equal society (p. 47). "Protective Democracy" (Model 1) thus gave way to "Developmental Democracy" (Model 2). Yet Mill's equalitarianism was undermined by his concern for safeguarding whatever development had already taken place: he gave extra electoral weight to the more developed members of society as judged by their education and station in life (p. 60). True, Lindsay, Barker, Dewey, and others elaborated on this model by supporting educational institutions for raising the level of social intelligence and instilling scientific attitudes. But their faith in human rationality was so much out of touch with empirically scanned reality that a new model–Model 3–readily emerged. This model of "Equilibrium Democracy" treats government as mechanism for responding to the expressed preferences of voters. It describes what in fact occurs in a society of elite-governed groups which make demands in the political market. It is an accurate expression of prevailing inequalities. If it fails to create the spirit and substance of community, it does provide a measure of affluence and protects society against tyranny.

At this point Macpherson's discourse smoothly glides toward Model 4–the ideal of "Participatory Democracy." How might an equalitarian, active, participatory society be realized? The acknowledged difficulty is that social inequality and individual apathy viciously reinforce one another. The elimination of one is a prerequisite for the elimination of the other. Fortunately we need not hold our breath: the circle is being broken. People are increasingly recognizing (1) the costs of ever-increasing economic growth, (2) the need for neighborhood action and work-place decision making, and (3) the inability of corporate capitalism to cope with economic crises and meet consumer expectations. "The prospects for a more democratic society are thus not entirely bleak (p. 106).

The voice is reasonable, the tone polished, the breathing steady. It is a persuasive performance, one so effective that it may remind us that manner rather than matter tends to be recalled even from the very best of seminars. In that case, of course, our prospects would be appreciably more bleak. However much we may share Macpherson's hope for a community of active members, we will have learned how to stay calm and collected. His measured prose and unqualified sobriety will serve, like well-edited book reviews, to keep even those of us who applaud him in our assigned place.

HENRY S. KARIEL

University of Hawaii

Harold D. Lasswell on Political Sociology. Edited by Dwaine Marvick. (Chicago: University of Chicago Press, 1977. Pp. vi + 456. $22.00.)

By several measures, Harold Lasswell will rank among the foremost social scientists of the recent past, the present, and the immediate future. The volume of his work alone commands attention: Dwaine Marvick estimates that 4,000,000 published words flowed from Lasswell's pen in the first 50 years of his scholarly career, more have come since his 1973 retirement from the Yale Law School, and still more remain to be published. Breadth of work, another criterion of significance, also testifies to importance; Lasswell's scholarship ranges over goals-clarification, trends-recording, explanatory analyses, developing projections, and alternatives formulation about contemporary and impending issues that span the social sciences and intersect with the physical sciences, especially as these issues confront the legal and medical professions. The originality of Lasswell's work counts perhaps most of all: his sweep embraces elite analysis, the invention and use of "developmental constructs," political communications, political psychiatry, and the role of intellectuals in political processes.

These five major themes constitute the outline that Marvick follows in organizing more than 300 pages of Lasswell's writings on political sociology. The 22 selections that comprise this volume, one in a series on "The Heritage of Sociology" inspired by Morris Janowitz, are drawn from Lasswell's early, middle, and recent years. Some are from his books, others from articles. Most are well known; what gives the collection special significance is Marvick's judicious editing.

Lasswell has often repeated himself, partly because he has never had the right audience. His real audience, as Leo Rosten once noted, is himself. His conventional audiences have been varied. Marvick excises the repetitions and aims to produce a coherent whole.

I have little doubt that the book will be useful in diffusing Lasswell's work among many who would not otherwise come upon it except in splintered pieces. Much of the unity that underlies Lasswell's work is captured by recasting material from diverse sources, written at quite different times. Marvick's long (72-page) introduction is the best single piece I have read on Lasswell; it surpasses essays by David Easton, Bernard Crick, Herbert Storing, and the several informative and analytic chapters in Arnold Rogow's *festschrift.* Certainly it tells the reader more about Lasswell personally than anything yet published, although hardly enough to meet the intensive and extensive demands of the founder of "the policy sciences" who would focus on culture, class, interest, personality, and level of crisis as the major variables in assessing a career in its context.

Lasswell has labored prodigiously and for a longer period than any of his contemporaries; the editor has selected carefully; would that the renowned publisher might have corrected page proofs with equal care. Misprints beginning on the second page and continuing through the index are not what readers have learned to expect from a university press distinguished, among other reasons to be sure, because now it reprints work begun by one of the university's celebrated alumni of nearly 60 years ago.

JAMES A. ROBINSON

University of West Florida

Hegel's Phenomenology of Spirit. Translated by A. V. Miller. (Oxford: Clarendon Press, 1977. Pp. xxxv + 595. $29.50.)

Hegel looms large in the history of modern philosophy, and among his works the *Phenomenology of Spirit* holds a special significance and indeed a fascination of its own. Its rather vast political implications were elucidated once again by Judith N. Shklar's recent work on the phenomenology in her book entitled *Freedom and Independence.*

In the light of the notorious shortcomings of the standard 1910 Baillie text of the *Phenomenology,* last revised in 1931, a fresh translation of the work has been urgently needed as essential support of the apparently still growing renewal of interest in Hegel's teaching. A. V. Miller gives us that translation. His rendition of the J. Hoffmeister edition of the *Phenomenology of Spirit* (not *Mind,* as Baillie has it) attains and indeed sustains throughout a truly astonishing clarity and continuity of style. Miller's decision to sacrifice "rigid consistency" for the sake of greater and more helpful clarity succeeds in avoiding the awkward sentence structures and obfuscating phrase convolutions characterizing much of the Baillie text. (This is not to deny, by the way, the often greater technical accuracy of Baillie's reading of Hegel's language. Baillie, for instance, renders Hegel's "jetziger *Standpunkt* des Geistes" in the Preface as *"position and point of view"* of spiritual culture at the present time. Miller translates *Standpunkt* simply as *state.* Also, Miller continues the habit of translating *Volk* as *nation,* rather than *people,* which Shklar, in admonishing Baillie, had suggested seemed the better translation.

Special mention must be made of the nearly 100 pages of concise paragraph-by-paragraph summary and analysis of the entire text by the noted Hegel scholar J. N. Findley. This particular mode of analysis can be of particular use to the beginning student or perhaps the political theorist seeking an initial broader philosophical context for Hegel's political philosophy.

A. V. Miller's superb translation will easily become the standard text in use as soon as it can be financially afforded by impoverished students and scholars.

HORST MEWES

University of Colorado, Boulder

Between Faith and Reason: An Approach to Individual and Social Psychology. By Francisco José Moreno. (New York: New York University Press, 1977. Pp. xvi + 133. $8.95.)

This is a book in the genre of Freud's *Civilization and Its Discontents,* but without Freud's redeeming sense of the complexity of human events. Its fundamental principle is pseudo-Kantian, namely that human beings are driven to ask questions which they are unable to answer. The sense is Kantian in that it understands humanity to be engaged in activity for which we can provide neither a solution nor a satisfactory stopping point; it is pseudo-Kantian in that Moreno sees the source of this dilemma in the simple fact that people are thinking beings. Kant had located the source of the dilemma simply in individuals. Contrary to what Michael Weinstein asserts in his introduction, Moreno sees it in the relation of reasoning persons to the world. Crudely, we look around and find that there are things which we cannot explain.

The name Moreno gives to this situation—the recognition that there are things which are and, against all our efforts, remain unknown—is "basic fear." We arrive at this conclusion on page six. The rest of the book is an account of the fact that human beings refuse to recognize basic fear and, in all sorts of other activities (believing in freedom, love, marriage, family, sex, faith, religion, politics), have sought to avoid the direct confrontation with their condition. These observations are not particularly new, nor is it novel to claim that such activities can be reduced to neurotic refusals to look reality straight in the face.

So, as Camus remarked some while ago, *les hommes meurent et ne sont pas heureux.* I have no objection to that conclusion, I suppose, for it seems to me as much as fact as anything can be. I find, however, that on the basis of Moreno's account I am unable to see as anything except self-deception those moments of joy or profound sorrow which I have had concerning personal or political matters in recent years. In the end, for all the scope and ambition of his concerns, Moreno is unable to give an account either of people's lives or of their sufferings which rings true. I believe the reason for this stems from two sources, one stylistic, the second conceptual. Stylistically, Moreno uses the papal "we" as the premise of his argument, e.g., "we fear death more than we are willing to admit and thus in dealing with it we follow our traditional pattern of repression" (p. 11). The "we" here is curiously disembodied and allows Moreno to make assertions about humanity without reference to time, space, history or group. For Moreno, human beings are reasoning beings (I wonder what Moreno would do with some of the recent research on animal speech) and that is all there is to say: to be human means nothing more or less than that.

The notion of humanity got Kant into trouble, as Hegel was the first to point out; as Robert Paul Wolff has recently reminded us (*Understanding Rawls*), it still continues to pose a problem for a certain kind of philosophizing. It is related to the conceptual problem in Moreno: he understands society as something imposed on human beings from the outside. We are, for instance, "forced to become acquainted with . . . language" (p. 24). This is simply silly. Language is not imposed on us, any more than our parents are (though Moreno thinks parents are imposed, also). Moreno retains an antagonistic understanding of the distinction between individual and society; he thus tacitly has a vision of individuals "properly" existing outside of the phenomenal world. No wonder he thinks it a condemnation for men and women to live on what must be an earth of misfortune. In the end, his mode remains trapped in the theology he seeks to escape. He might have reformed his views somewhat had he remembered with Rousseau and Calvin that though this earth be a value of tears, it is also the New Jerusalem. After all, Blake saw eternity in a grain of sand and was the richer for it.

TRACY B. STRONG

Amherst College

Due Process. Edited by J. Roland Pennock and John W. Chapman. (New York: New York University Press, 1977. Pp. ix + 362. $17.50.)

This volume in the NOMOS series is divided into three parts. The historically oriented first part includes a lengthy and excellent essay, "The Forest of Due Process of Law: The American Constitutional Tradition" by Charles A. Miller and a less satisfying, though humorous, essay, "Due Process in England," by Geoffrey Marshall. Miller's essay traces the intertwined history of the procedural and substantive elements of due process through, e.g., the high-water mark for distinguishing between these elements which occurred in the Virginia Bill of Rights, and provides a fine background for the major part of this volume.

Valuable analytic contributions are made by Thomas Scanlon, Frank Michelman, Edmund Pincoffs, Thomas Grey, David Resnick, and Thomas Kearns. Each of these essays deals with the basis or character of procedural due process rights and/or of substantive due process rights. As a framework for the characterization of these authors' views, some formulation of the difference between the two sorts of due process rights is necessary. I suggest that a *procedural* due process right exists when the practice of certain types of procedures creates for an individual a normative claim that these procedures run certain specific courses, while a *substantive* due process right exists when a right which is independently possessed by an individual constrains practices to certain specific avenues or results.

The position of Scanlon's rich essay is that due process rights are founded on "the unacceptability of arbitrary power." Procedural due process insures that the power provided to those charged with carrying out certain practices is not used arbitrarily, while substantive due process requires that the practices carried out by powerful institutions remain in accord with the moral purposes which justify that power and thereby render it non-arbitrary. Michelman examines one particular understanding of the value of procedural due process—that it serves the associational values of consultation and participation. He is concerned with whether these non-formal values can be legally guaranteed by the creation of formal legal entitlements to specific procedures. Pincoffs proposes a Kantian reading of the basis for procedural due process, viz., that certain specific procedures have non-instrumental value in virtue of exemplifying the Kantian injunction never to treat anyone as a mere means. Grey plausibly insists that many due process demands ought not to be seen as procedural because, "they are ... designed to protect various substantive rights and interests." Procedural demands, strictly speaking, are all to be understood as being "designed to promote the correct decision of disputes." Thus, in contrast to Pincoffs, Grey offers a fully instrumentalist account of procedural rights.

For Resnick, due process requires eschewing both "unfair" and "unjust" procedures. Unfair procedures are those which are not effective for arriving at correct decisions. But arriving at correct decisions is not enough. People have rights to the best methods being used in arriving at decisions. These rights to "fair" procedures are, then, obviously tied to, but not entirely reducible to, the instrumental value of the procedures. In contrast, according to Resnick, unjust procedures are those which have "morally undesirable consequences." Resnick seems to include under just procedures all the procedures which Scanlon and Grey would characterize as procedures demanded by substantive due process rights. Certain claims which Pincoffs focuses on would, if recognized by Resnick, also be considered claims to just procedures.

Kearns' essay is directed against the explanatory value of various moralistic conceptions of due process and, thereby, against the sort of conceptions found in all the other authors in this section, according to which due process involves recognition of certain normative claims on behalf of individuals against various public institutions and practices. Kearns proposes that one can better explain the due process phenomenon in terms of the existence of "system" which cunningly uses the passions of individuals, resentful of destabilizing extensions of the power of "system," to set self-preserving checks upon itself.

The last portion of *Due Process* focuses on majority rule. Gerald Kramer provides a survey of alternative conceptions of majority will, their manifestations in various parliamentary procedures, and the complications introduced with strategic voting. Arthur Kuflik provides a systematic and masterful discussion of the supposed justifications of majority rule. Among the many valuable points and distinctions developed here is the ongoing distinction between instrumental and non-instrumental views regarding the value of majority rule which parallels disputes about the grounding of adjudicative due process. Richard Epstein points out that Kramer-like understanding of the deficiencies of various voting schemes should raise doubts about the due process stature of certain union elections. Epstein argues that if freedom of contract is to be abridged by the establishment of an exclusive union bargaining agent, then the courts might at least insist that the voting procedures for selecting that agent not allow minority victories.

These essays, along with J. Roland Pennock's useful introduction, constitute a valuable, informative, and thought-provoking volume.

ERIC MACK

Tulane University

The Marxist Conception of Ideology: A Critical Essay. By Martin Seliger. (New York and London: Cambridge University Press, 1977. Pp. xiii + 229. $13.95.)

Martin Seliger's concerns in this book are threefold: to outline and criticize Marx's theory of ideology, to trace the changes the theory has undergone in the hands of his followers, and finally, to offer an alternative view of ideology. The second task is competently executed, but does not break any new ground. The third is executed rather perfunctorily and raises more questions than it solves. The first is Seliger's basic task, and accordingly I shall concentrate on it.

Seliger detects several ambiguities in Marx's theory of ideology, of which two deserve attention. First, as Marx defines it, ideology has two features: it is class-conditioned, and it is a distorted perception of reality. In Seliger's view Marx did not establish a clear relationship between the two features, sometimes saying that distortion necessarily results from class conditioning and sometimes it does not. According to Seliger, the former represents Marx's well-considered view. Even this view contains an ambiguity, Seliger argues. Sometimes Marx says that class-conditioned thought is distorted only when relations of production come into conflict with forces of production. In Seliger's view Marx generally inclines towards the former non-evolutionary view of ideology.

Seliger criticizes Marx's view of ideology as class-conditioned and therefore distorted thought on rather pedestrian and unoriginal grounds. For Seliger, it is refuted by the fact that members of the same class sometimes subscribe to different ideologies, and, conversely, that those belonging to different classes sometimes subscribe to the same ideology. It is also refuted by the fact that different historical epochs can be shown to share several basic assumptions in common. Finally, Marx's view leads to the "well-known paradox" that in order to describe a body of beliefs as distorted, Marx would need to have objectively valid criteria of truth which, if all social thought is class-conditioned, he simply cannot develop.

Although Seliger's book contains several interesting insights and perceptive observations, it is on the whole rather disappointing for two basic reasons. First, Seliger is never entirely clear on what he takes Marx's theory of ideology to be. Sometimes he says that, for Marx, social and political ideas are determined by "reality" (whatever that may mean), while on other occasions they are said to be determined by "social and economic conditions," "social relations" or, more narrowly, "class." For the most part, Seliger singles out class as the decisive factor. Again, we are never clearly told what it is that is so determined. Sometimes all thought is said to be class-determined, whereas on other occasions all "social and political thought" or all "socially relevant" thought is said to be so determined. Seliger is equally unclear on the relationship between class on the one hand and social and political thought on the other. Sometimes the latter is said to be "determined" by class, and at other times to be "conditioned" by, or "derivable" from, or "dependent" upon it. The failure to articulate Marx's thesis is disturbing enough. Even more disturbing is the fact that almost all of Seliger's criticisms are directed at the thesis that all social and political thought is "exclusively determined" by class. Since Marx only occasionally took this view as Seliger himself acknowledges (pp. 53 ff., 203 ff.), his criticisms remain pointless and ineffective.

Second, like most other commentators on Marx, Seliger makes no attempt to locate the epistemological context within which Marx's theory of ideology arose and the perplexities which it was designed to illuminate. Like all great rationalist philosophers of the past, Marx was an epistemological objectivist, and never doubted that we can arrive at objectively valid knowledge about social and political life. In the course of his analysis of Hegel, classical economists and others, Marx made an important discovery. He found that the social theories developed by these writers selected, organized and interpreted facts in a manner that showed not merely occasional personal prejudices, blind spots and failure of imagination, but a *systematic* bias or orientation of thought. Marx called such systematically biased theories ideologies. For him, ideology is not so much a false as a systematically biased theory. Marx asked how such a bias could be explained. He answered the question in terms of the bias inherent in the conceptual and methodological apparatus of the tradition of the discipline in question, and explained this bias in turn in terms of, among other things, the social basis of the tradition. Seliger's query as to how Marx could arrive at an objectively valid theory of society is therefore misplaced. For Marx anyone gifted with intelligence and impartiality can arrive at the truth; the problem that worried him was not why some did but why some did not.

For Marx the determination of the ideological character of a theory and of its social origin are distinct levels of inquiry. That is, ideology and sociology of knowledge should not be confused. Marx first subjects a theory to critical examination to ascertain *if* it reveals a systematic bias, and *then* seeks to explain the origin of the bias in social terms. The ideological character of a theory is not deduced from its originator's social background as Seliger maintains, but ascertained and demonstrated by means of a careful philosophical analysis. The sociological inquiry is *subsequently* brought in to explain the bias discovered by means of a prior and independent philosophical analysis. Seliger conflates the two levels of analysis and misses the essence of Marx's theory of ideology.

BHIKHU PAREKH

University of Hull, England

Government and the Mind. By Joseph Tussman. (New York: Oxford University Press, 1977. Pp. viii + 175. $8.95.)

The publisher describes this book as "controversial and passionate" and promises that it will "fascinate" a diverse audience. I suspect this would not be found deceptive advertising, but in addition Joseph Tussman's essay is alternately committed, uncertain, hopeful, fearful, temperate, shrill, wise, and not so wise. In sum, it exhibits most of the qualities we would expect in the troubled but defiant confession of faith of a liberal professor reflecting on the challenges of the last 15 years to the public institutions embodying his convictions. And the central institutional vehicles of liberalism receive consideration here: market, church, state, court and school, together with the cluster of concepts each represents: choice, conscience, sovereignty, justice, and education. Indeed, it is no injustice or disparagement to say that Tussman has written an extended footnote to *On Liberty,* or that he is implicitly engaged in a dialogue with its author.

If only from its title, Anglo-Americans would expect this to be a "First Amendment" book, and that expectation would be accurate in the sense that that constitutional provision *frames* the argument. Between the question: "whether government . . . has (or may be given) authority to act in this area of concern, or whether, for special reasons, the community may not act through government in matters which concern the mind" (p. 11), and the answer: "government is to lead, to discern needs, to formulate, propose, persuade, to enter as an active partner into the deliberative life of the community" (p. 115), dwells Tussman's muse. He is particularly and properly concerned to remind us that free speech does not give rise to Babel, that it requires the organizing, participant presence of government: "The forum is not something we have before government takes a hand and which is 'free' until government intrudes. It is a system in which government is a constitutive element" (p. 99); or, more strikingly, "Our paradigm of free speech is a highly structured artifact" (p. 100). The classical theme is clear: civilization equals public discourse which requires a forum, and this implies consensual parameters, the symbolic walls of the city, as Arendt has put it. "Politics" is then understood as the process of communicating about public matters within these walls, and the more refined task of the philosopher is to make the hard and largely incremental judgments of application: who should speak? for how long? when? where? Political philosophy, in short, becomes the province of the "tough distinction makers" rather than the "apocalypse kids."

But these latter juveniles (their youth reflecting a state of mind as well as their age) worry Tussman. They appear, in a different metaphor, as "invading barbarian[s]," "gaping" incomprehendingly over the walls of civilization (p. 125) and in more contemporary guise as "those who are not yet spiritually toilet-trained" (p. 91, The Berkeley Free Speech Movement?). We are repeatedly reminded of the crassness of our quality of life, but Tussman chooses to stress its *incivility* rather than, as Marcuse might put it, its *exploitative* character. Encapsulated in Tussman's lament is a large part of what many more ponderous books have labored to diagnose as the waning of liberalism (the New World version of Spengler?) and which Walter Lippmann described as the demise of the public philosophy, a culture of restraint and civility. The difficulty, as Tussman admits, is that at a crisis stage of this sort there is very little, either in his view of that of liberalism, that government can remedy, and this leads him to assert both the eternality and the fragility of authority, its Enlightenment bases and its arcane dimensions: "a 'King' is never naked and . . . it takes imagination, understanding, and discipline to see the otherwise invisible but real clothing" (p. 72). Further, "Respect for government, monarchic or democratic, respect for law in its mysterious sovereign majesty, respect for the constitution, may be its [a troubled community's] only hope" (p. 89). But this latter is the stuff of political sociology, not philosophy, and the inverted commas in the former quotation speak eloquently of the embarrassment we suffer at approaching the issue of legitimacy. We respect authority because we understand it or because we do not; the Royal Lie and the marketplace of ideas are not such ready bedfellows. In this case it is not so obvious that the priestly mode has no need of the prophetic, and what other lesson do the 1960s hold for us?

If the 1960s do not offer a lesson, they surely pose a question, as Tussman has boldly recognized. In effect he asks us to accept that *some* authority must order public life, and invites the question as to which version we would prefer: a modestly "democratic" rulership (presumably made responsible by the ballot) or the private bureaucracies of General Motors or OPEC? Thus put, most of us could probably respond with minimum cognitive or emotive stress, but the task facing Tussman is rather more formidable. Whether appalling by massive or merely statistical significance the extent of public disaffiliation from the political order (alienation, anomie, feelings of helplessness or rage, call it what one will) can hardly sustain the robust and tolerant civic life he envisions as "near the top of the scale . . . the great deliberative assembly in which virtue and wisdom display themselves calmly in the dignified service of the common good" (p. 124); or even its degenerate form: a "bargaining transaction," which "understands mutuality," "accepts interdependence," and "is a possible mode of decency" (p. 125). Paradoxically the younger Mill recognized the necessity for that mysterious entity "consensus" to knit together the pluralist strands of his social universe, but was inhibited for that very reason from generating a unifying vision (as, for example, did Hegel) of a distinctive political *ordering* function. Again, the imperatives of market, church, court, and school, are not so easily made additive to "polity," and especially not when a primary theoretical directive is to "limit" the sphere of state activity.

It is an index of this difficulty that so sophisticated and thoughtful a scholar as Joseph Tussman needs frequently employ such words as "sane" and "sanity" to establish both the boundaries and the conduct of discourse within the forum. The poltergeist haunting liberalism's banquet need not materialize in the brutal use of Soviet psychiatry to silence and torture dissidents; as Lindner in the past and more recently Szsaz and Foucault have noted, involuntary and community mental health programs are not without a political patina. The dangers of legislating sanity or of appealing to a standard of sanity to establish the limits of the permissible have long been manifest, but it is perhaps unfair to both Mill and Tussman to demand more than recognition and excavation of this darker side of liberalism.

In sum, this essay casts an unflattering light on a large part of our cultural as well as our political heritage while insisting that this heritage remains a viable moral foundation for public life. Tussman's perspective engaged my interest, and, I believe, most readers will find it as interesting as I did.

Paul F. Kress

University of North Carolina, Chapel Hill

Punishing Criminals: Concerning a Very Old and Painful Question. By Ernest van den Haag. (New York: Basic Books, 1975. Pp. xiii + 283. $11.50, cloth; $3.95, paper.)

This book is a contribution to what has been called the "rehabilitation of punishment." For at least several decades, punishment *qua* punishment was out. The philosophy of punishment as retribution was thought to be unsound and/or a rationalization for vengeance. Moreover, it was argued that deterrence did not work. Sociologists argued that because crime was caused by underlying social pathologies (e.g., poverty, inequality, differential association), threats of punishment would not deter offenders. Psychoanalysts argued that because crime was caused by underlying psychopathologies, threats of punishment would not deter offenders. In general, the rehabilitative model of punishment dominated academic and penological thinking.

In the last decade or so, punishment has become more popular. Social science studies questioned the efficacy of rehabilitation, and the economic analysis of crime suggested that the criminal behavior would increase in response to increases in price. Among philosophers, there was a renewed interest in Kantianism and retributive defenses of punishment. The soaring crime rates of the 1960s strengthened the demand for punishment among some of the public, while the Watergate scandal made punishment more palatable to others.

In this context, Van den Haag defends the moral legitimacy and empirical efficacy of punishment. His philosophical argument is that society has a moral right and obligation to punish criminals. The empirical argument is that whereas rehabilitation does not work, punishment does deter crime and would deter more crime if we were to get tougher. Although the book's organization is marred by the author's tendency to jump back and forth between various theses, the arguments are abundantly clear. Written for the informed lay person, van den Haag's book paints its thesis in very broad stokes. Although this makes for lively reading, the author is not aware of some of the difficulties in his argument, and, on occasion, he makes some glaring errors.

The book begins weakly. Van den Haag argues for society's right to punish criminals. He states that "conduct prohibited by law is wrong..." (p. 4). Now this is either tuatologically true (if he means that such conduct is legally wrong) or false. He goes on to argue that since "we all benefit from even the worse social order . . . we all owe society some allegiance in exchange..." (p. 16). This assumes what has to be shown, namely, that everyone does benefit (or has a *net* benefit) from the worst social order and that such benefits create obligations. He also argues that society has an obligation to punish criminals because it has promised that it would do so. Once again, this argument is problematical; one cannot create an obligation to do something simply by promising to do it, unless one has a right to make that promise in the first place. Moreover, a person to whom a promise is made normally can cancel the obligation to keep the promise. Since the criminal obviously cannot do so, that version of punishment as a promise seems to be not very helpful.

In a standard move, van den Haag attempts to resolve the conflict between retributive and utilitarian theories of punishment by arguing that "utility must prevail" at the legislative phase of making laws and prescribing punishments, whereas retributivism must prevail when distributing punishments (p. 25). Unfortunately, he does not indicate just why retributivism would be wrong at the legislative phase, and apparently contradicts himself when arguing that "our idea of justice . . . must be prior to, and independent of, the laws we make in accordance with it. . ." (p. 34). On the other hand, he does not indicate why a utilitarian principle of distribution is incorrect, i.e., why we should punish someone when such punishment will not be useful (including the effect on the credibility of the threat of punishment). He errs again when he argues that juveniles should be held responsible for their crimes because the "victim of a fifteen-year-old mugger is as much mugged as the victim of a twenty-year-old mugger. . ." (p. 174). Perhaps juveniles should be held responsible, but this argument does not show it.

Van den Haag is stronger and more insightful on the empirical questions. He presents some plausible arguments in defense of the deterrence hypothesis and is at his best when debunking some of the common objections leveled against that thesis. He shows that crimes of passion are preventible, and that deterrence does not require that the criminal consciously calculate the benefits of crime and the costs of punishment. There is a nice discussion of the differences between crimes which are responsive to incapacitation and crimes which are not.

Although there are no major theoretical or empirical breakthroughs here, many of the arguments are important. One hopes that van den Haag's lack of caution and subtlety will not cause his valuable points to be ignored.

ALAN WERTHEIMER

University of Vermont

Teaching Political Science: The Professor and the Polity. Edited by Vernon Van Dyke. (Atlantic Highlands, N.J.: Humanities Press, 1977. Pp. ix + 179. $10.00.)

This volume, consisting of papers delivered at a 1974 Shambaugh Conference, deals with a series of related but by no means identical questions. Among these are the appropriate attitudes which teachers of political science should take with regard to (1) the political order, (2) scientific and moral issues, (3) public policies, and, at a manifestly different level, (4) "how to make teaching better." As might be expected, the contributors raise and answer quite different issues.

In the opening essay ("Mainstream Political Science and Its Discontents"), Stanley Rothman discusses both the major attacks (by the Straussians and the New Left) on "establishment" political science (represented, in Rothman's view by Almond, Eulau, Berelson and, perhaps, Dahl) and the overlapping question of whether the political scientist can be "objective" in conducting research and/or in teaching. With regard to the former, Rothman concludes that, while the critics "have made some telling points, they have not made their case." On the latter, he opts neither for scientific objectivity, nor for total moral involvement, but for a "disinterested (but compassionate) search for truth."

The next paper ("Education in Political Commitments") by Peter Bachrach and Douglas Bennett, maintains that students should be taught to "formulate commitments in political science classrooms." This requires that the teacher share with the student his/her own political views and any "doubts and unresolved problems about them." The contention that such commitments are necessary aspects of good political science teaching would be more persuasive if the authors had given some specific evidence of their success with this type of pedagogy.

Heinz Eulau's "The Politicization of Everything: On the Limits of Politics in Political Education" attacks "the Proposition," i.e., the notion "that the teaching of political science is itself a political act." In Eulau's pungent prose, "the Proposition is logically absurd, theoretically trivial, empirically false, ethically undesirable, morally wrong, and politically dangerous." In the final analysis, the moral posture of the teacher should be that of an individual "whose knowledge presumably places him in a better position to discriminate between student wants and needs than students themselves are able to do."

Martin Landau ("The Classroom and Political Science: An Essay on Political Theory") is equally critical of the proposition and equally concerned by the possible consequences of its acceptance within the profession. Landau's position, stated with some nice touches of humor and irony, in many respects parallels that of Eulau; at other points it is developed in somewhat more formal philosophical terms.

Martin Diamond ("The Teaching of Political Science as a Vocation") reminds the reader that the tension between politics and science is so great that Max Weber saw the two as belonging to wholly separate vocations. The balance of Diamond's essay is devoted to two theses: first, that the Weberian principles are applicable only in a liberal polity; second, that it is essential to go beyond these principles. This can only be accomplished, Diamond concludes, not surprisingly, by obeying Leo Strauss' injunction that we liberate ourselves "from the supposition that value statements cannot be factual statements."

Allen Bloom opens his discussion of "Political Science and the Undergraduate" by contrasting contemporary political science with that sketched by Aristotle, much to the disadvantage of the former. To recapture the interest of the undergraduate student, we must re-emphasize political philosophy, provide the student a solid grasp of history, and give greater attention to the perennial problems encountered in the study of international relations. The malaise of undergraduate political science needs no radical remedy—"just teachers, students, books, and a calm setting in which rational discourse is respected."

Henry Kariel ("Becoming Political") opens with the assertion that "Hegel surely had it right." Unfortunately, the question which Hegel was addressing, let alone his answer, is never made quite clear. Similarly, Kariel's aphoristic style often left me puzzled as to just what topic he was addressing at any particular moment. The explanation to this may be found in Kariel's statement that "my courses, seminars, meetings, and publications have become increasingly choppy, fragments of an unknown whole."

The penultimate essay, by Vernon Van Dyke and Lane Davis, deals with "Values and Evaluation in Teaching Political Science." The basic objective of political science teaching "should be to help students make better decisions." This requires that political scientists give attention both to normative principles and the problem how best to subject these to critical evaluation and assessment.

Martin Kilson's "Political Science and Afro-Americans: Normative Problems of American Politics" is the last, and shortest, essay in the volume. It pays relatively little attention to the questions on which the other contributors focused, but deals instead with the problems of black-white cleavages.

Plainly, it is difficult to characterize, let alone summarize, a book of this sort. Many of the issues discussed are overly familiar to political scientists and there is relatively little in the way of argument that will be novel to those who have kept abreast of the literature. The book, according to Van Dyke, "is addressed primarily to teachers and students of political science." The former will undoubtedly find it useful; I am not as sure as I would like to be that it will do much to attract many of the latter.

ALBERT SOMIT

State University of New York, Buffalo

American Politics

Lobbying for the People: The Political Behavior of Public Interest Groups. By Jeffrey M. Berry. (Princeton, N.J.: Princeton University Press, 1977. Pp. 331. $20.00, cloth; $5.95, paper.)

In this ambitious and ground-breaking study, Jeffrey Berry succeeds in accomplishing one objective: "to expand the basic knowledge on public interest groups and to integrate the findings into the larger body of literature on interest groups" (p. 4). He succeeds less well in his second objective: to understand "the process by which public interest groups reach decisions concerning their issue priorities and their lobbying tactics" (p. 5). In general, these two purposes, descriptive and explanatory, often seem at odds. The book's descriptive value is limited by its attempt to categorize observations in a loose conceptual framework; the explanatory value is limited by the adequacy of the conceptual framework itself and by a lack of follow-through between the framework and the case studies. As a preliminary mapping of underexplored territory, nevertheless, the book is a very valuable contribution to the field of interest group behavior.

The descriptive value of the book derives from both extensive interviewing and participant observation. The ambitious survey covered 83 Washington-based "public interest" organizations, including, for example, the American Civil Liberties Union, the Center for Auto Safety, the Children's Foundation, Common Cause, Consumers Union, the Environmental Defense Fund, Sane, Young Americans for Freedom, Zero Population Growth. In addition, Berry drew on apparently extensive personal participation and observation to produce two fine case studies, of the Fund for Animals and the Women's International League for Peace and Freedom. Some of the substantive findings on the entire sample are concentrated in chapters on organizational resources and activists (the latter, chapter 4, makes a number of interesting comparisons between the characteristics of public interest advocates and Milbrath's Washington lobbyists). Other observations on, for example, communications between activists and constituents seem palely reflected in the loose conceptual frameworks that are meant to highlight the various determinants of group formation, maintenance and decision making.

Separate conceptual frameworks are employed for, on the one hand, group formation and maintenance and, on the other hand, a group's decision-making process that leads up to the selection of advocacy strategy and tactics. Chapter 2 tests alternative theories of group formation and maintenance: Truman's "disturbance" theory that views a disruption in social relations as a precondition for formation and Salisbury's exchange theory that focuses on the role of the entrepreneur. After what purports to be a test of these alternative views, Berry concludes that "the entrepreneurial theory of group origins has greater explanatory power for public interest organizations" (p. 25). The test, however, is unsatisfactory and the reasons lie in the nature of the cases used. Given a sufficiently large group of both members and leaders, one might be able to determine the extent to which the group arose because of a disturbance, experienced by incipient members, or because of a leader's entrepreneurial skills. Berry's test, however, is partly based on organizations that cannot be considered "groups" in any serious sense; 25 percent of the organizations did not have more than one full-time, paid professional staff member, 30 percent had no membership, and 57 percent of the membership organizations had no mechanism for eliciting or considering membership option. It is not surprising that interview responses for most of these organizations, many of which consist solely of a handful of activists, reflect the dominance of entrepreneurial activity. Moreover, interviews with leaders should not be relied on as the sole measure of leadership activity; on this issue in particular there is a serious need for corroborating evidence. In addition, elite interviews probably are limited as a device for identifying social disturbances; this determinant might better be made by the analyst, not the advocate.

In a much more tentative vein, Berry proposes a separate conceptual framework to highlight the determinants of group decision making, of decisions about advocacy tactics in particular. Advocacy choices (litigation or personal lobbying, for example) are shaped by two classes of factors: organizational goals and capabilities (staff resources, etc.) and the structure of the environment (legal definitions of standing, etc.). Although proposed as a device for synthesizing the findings of previous chapters, not much use is made of this conceptual framework. Here again, the problem may lie with the data base. Many of the included groups are, organizationally, exceedingly simple and, as Berry notes, the activist simply makes, in effect, an "automatic" response in decision situations. For many of these organizations, and in particular for the two that serve as the subjects for the case studies, the constraints of scarce resources and an unreceptive environment appear to render serious strategic thinking moot; consequently, the dynamics of decision making are not revealed and the potential value of the conceptual framework is unexplored.

PAUL A. DAWSON

Oberlin College

The Adoption of Innovation by Local Government. By Richard D. Bingham with Thomas P. McNaught. (Lexington, Mass.: Lexington Books, D.C. Heath, 1976. Pp. xi + 271. $19.00.)

The processes by which local governments adopt new practices (innovations) have recently become a subject of intense inquiry. This is because the local public sector has been perceived as a laggard in adopting innovations. In general, local agencies appear unresponsive to the enormous changes in demands for services; they also seem not to have used technological capabilities available across the country.

Richard Bingham's volume adds significantly to our growing knowledge about local innovation processes. His study covers eight different innovations (e.g., the use of several different computer systems, of vehicle management services, of prefabricated housing, and of videotape recorders) in four different types of agencies (public housing authorities, schools, libraries, and general purpose government). For each innovation and in each service, he has tested the same general model of the innovation process. Bingham finds that the adoption of innovations results mainly from a complex set of organizational and community factors, and his model discriminates well between the direct effects (mainly organizational) and the indirect ones (mainly community, and stronger than expected). A key finding is that innovations can result from *amenity* as well as *need* conditions.

As with other research monographs, this volume is likely to serve the research specialist better than the student. However, Bingham's methodology and model-building analysis do provide the student with an exemplary set of procedures that are rising in importance in empirical social research:

First, for every type of innovation, Bingham's data base combines a massive mail survey (with response rates exceeding 90 percent) with a set of case studies. The mail surveys covered the relevant agencies in the 300 or so cities with 1960 populations over 50,000. The case studies were selected to illustrate the major processes in a qualitative manner. The use of two different sets of data, collected independently but addressed to the same set of issues, should be more commonly used in future studies of organizational behavior, and Bingham's work sets a good example.

Second, Bingham consistently tests a process-oriented model that has been specified at the outset. In too many similar studies, such specification has not been made, and investigators have merely made interpretations of process on the basis of an unguided series of correlations. Bingham's model consists of four factors—community factors, demand factors, organizational characteristics, and features of the organizational environment. For each factor, many variables and measures were examined (e.g., amount of federal aid as a measure of organizational environment, number of practitioners as an organizational characteristic, SES as a community factor, and school dropout rates as a measure of demand for school services).

Third, the volume contains a clear tabulation of the hypotheses tested and the general pattern of results. This is an extremely difficult task when dealing with a diverse array of innovations and agencies, and again students are likely to learn much from the study about the research process of synthesizing and aggregating a series of findings.

Overall, the volume is well composed and well written, but somewhat mechanical in its structure. An introductory and concluding chapter serve as the bookends for four chapters, one for each type of agency. However, the organization of this type of material is not easy, and the four agency chapters at least follow parallel outlines. It is also quite likely that the four agency chapters will facilitate the work of research specialists, because audiences interested in public housing, public schools, libraries, or general purpose governments can each concentrate on their chapter, complemented by a reading of the introduction and conclusions.

Empirical studies of organizational innovation are becoming more sophisticated. No longer is it acceptable to draw conclusions based only on single types of innovations or on non-nationwide samples. Similarly, the state of the art has also progressed beyond the crude arraying of specific correlates without some overall process model. The Bingham book shows how these traditional shortcomings can be systematically overcome.

ROBERT K. YIN

The Rand Corporation

Attitude Psychology and the Study of Public Opinion. By Forrest P. Chisman. (University Park: Pennsylvania State University Press, 1976. Pp. xvi + 253. $13.50.)

While criticism of the voting studies may still be in vogue, this book which purports to be a devastating attack is hopelessly shallow, anachronistic, partially inaccurate, and nonconstructive. The first part discusses two intellectual traditions of public opinion: the political philosophers and the attitude psychologists. It is primarily definitional/descriptive in character, and duplicates widely known and available sources. The major sections which follow treat two general points: (1) that the role of issues in voting has been neglected and inaccurately conceived—this is fundamentally an "issue publics" argument that only one of a few issues may be important to the motivational scheme of voters; (2) that research on linkages between opinion and policy is inadeuqate since the correspondences must be examined reciprocally for their paths of influence.

Chisman offers a heavy dose of philosophical, as opposed to theoretical, perspectives aimed primarily at traditional assumptions underlying studies which are rooted in normative thought. Although the title suggests an emphasis on psychological criticisms, perhaps even a new theory or a recombination of old social-psychological concepts, these are not forthcoming. Little emphasis is placed on offering alternative theories or models of voting and public opinion. Since this is a critique largely devoid of new ideas or methodological suggestions, we should expect the review to be both thorough and contemporary. Yet the book focuses almost exclusively on *The American Voter* and Key's *Public Opinion and American Democracy*—nearly chapter by chapter—and generally informed readers will find little new here. Indeed, in comparison to many critiques, replications, and extensions of the Survey Research Center tradition, the book is shallow and hopelessly out of date. Five chapters devoted to *The American Voter* attempt to counter the argument that "issue attitudes have no influence" and that "party preference is the major determinant of votes," i.e., that "motives" for votes are inadequately identified. These charges are only partially correct and somewhat irrelevant (in terms of the scholarly tradition), given subsequent research developments which go unexamined. There are no article citations in the book beyond 1972 and only about 10 percent go beyond research since 1968. Therefore, the book is almost totally devoid of "revisionist" research. As Chisman attempts to develop an issue critique, he fails to discuss or cite more than one (RePass) in the recent period and has no elaboration of the work relating to issue publics which is so essential to his argument. Chisman is apparently not even aware of Rossi's old essay. There is no discussion of the rational choice alternatives, nor a comprehensive treatment of the attitudinal components model. While he "fusses" about the relationships between partisan identification and party attitudes, including the direction of causality and incorrect "stability" findings, there are no citations to attitudinal component model replications, to any of the socialization literature, or to any longitudinal study.

With the exception of a summary, the remainder of the book is devoted to a critique of Key's model of opinion-policy linkages. What appears to be a sweeping indictment is characterized by common faults found throughout other parts of the book. Students should find other sources which present several alternative interpretations (e.g., Luttbeg). As with his treatment of issues, Chisman, unlike political scientists, ignores changes in the political environment over time and the role of mediating institutional forces. He is so out of touch with contemporary political science as to imply that scholars study public opinion as if it were a good substitute for elections. But few in the discipline have expressed this level of professional naivete over the last 25 years.

Two redeeming features of the book are its organization and Chisman's attempts to present some balance by periodically mentioning possible objections to his argument. Nevertheless, the text is redundant and the author does not adequately marshal data/findings to support his arguments. No attention is paid to scientific criteria for testing rival hypotheses, and the author appears to have a relatively weak grasp of research design and analysis considerations, as well as the distinction between levels of analysis.

This book will be unfulfilling to students of many persuasions. Those desiring scholarly ammunition for attacking the voting/opinion studies tradition—from either a normative or empirical perspective—will find it superficial and ordinary. The philosophical critique is not rigorous in that tradition, and the empirical analysis is naive. In addition, those seeking constructive suggestions to correct the apparent morass claimed by the author will find little guidance in it. Even empirical theorists of the psychological persuasion will note that the discussion fails to rise above the introductory text level. As much as "second generation" students of mass politics may labor to engage the author's biases, even those most critical of "first generation" works will find little of assistance. Indeed, the inadequacies of this book will place even the boldest critics on the defensive. The grand irony is that amidst all of the charges that Campbell et al. were "wrong" and the author's admission that the data were adequate, he fails to bolster his arguments with subsequent reanalyses of the data provided by the Survey Research Center/Center for Political Studies/Interuniversity Consortium for Political and Social Research in the spirit of open inquiry.

SAMUEL A. KIRKPATRICK

Texas A&M University

Sex Roles in the State House. By Irene Diamond. (New Haven: Yale University Press, 1977. Pp. xii + 214. $12.50.)

Irene Diamond examines the role of women in state-level politics from two perspectives. First, she looks at the broad contours of women's representation in the state houses in order to determine the pattern of female participation nationwide. Diamond posits that political competition, defined as the desirability of the office and the status of the office, is the major determinant of the sex ratio in the legislatures. She analyzes aggregate-level data in order to test this fundamental relationship and then introduces survey data to determine the impact of competition on the legislators' career pattern, political ambitions, and legislative performance. The second focus of this study centers on women legislators and their role orientations. Relying on in-depth interviews, the author identifies four types of women officeholders and then discusses the political consequences of their presence in state houses.

The first part of the book is by far the strongest, both conceptually and analytically, and is an exciting contribution to the literature on women in politics. Diamond hypothesizes that political competition determines the proportion of women represented in the legislative arena and also influences the career patterns of these women. She begins by positing an inverse relationship between competition and sex composition in the state houses: the more desirable and prestigious the legislative offices, the fewer the number of women. Using data from 1971, Diamond tests this proposition at the aggregate level and finds that it holds. In addition, her analysis shows that there is a high incidence of women legislators in the New England states, a region also classified as noncompetitive, in contrast to the southern states which persistently under-represent women. Diamond suggests that the cultural traditions in these regions are largely responsible for facilitating (in the case of New England) or hindering (in the case of the South) the entry of women into state-level politics. The analysis of regional variations is innovative and thought-provoking, but the cultural explanation offered remains largely speculative and unconvincing.

Diamond then presents an overtime comparison of the sex composition in the 50 states. She finds that the sex ratio in the state houses in 1961 is the best predictor of the sex composition in 1971. While the absolute number of female legislators might have increased from 1961 to 1971, the proportion of women in each of the state legislatures remained relatively stable during this time. When the author introduces data from 1975, however, she finds that in certain states there is a noticeable increase in the proportion of female legislators from the earlier period. This situation suggests that the translation of political competition to seats has changed. Diamond attributes the change to the impact of the women's movement. This is a reasonable hypothesis but it remains untested. It would have been useful had the author included a measure of women's organizational activity in each state to see whether the feminist movement accounted for the increased proportion of female legislators in these states.

Once having established the basic relationship between competition and the sex composition of the legislatures, Diamond turns to an examination of the impact of political competition on the career patterns of state officeholders. She hypothesizes that the level of competition for office will attenuate sex differences. For this analysis the author surveys legislators from four noncompetitive New England states and treats competition in terms of district-level as opposed to state-level characteristics to increase the variance. Diamond recognizes the constraints imposed by her sample design and offers her results as only tentative. She finds that at both ends of the competition continuum the characteristics of male and female public officeholders are similar. The noncompetitive districts are monopolized by the amateurs while the professional, career-oriented politicians dominate the competitive seats.

Up until this point Diamond has concentrated on identifying the pattern of female participation at the state level. Her concluding section addresses the question of whether the type of women recruited to public office, rather than the number of women, makes a difference for the representation of women. She uses role theory to develop a fourfold typology of female legislators. The discussion that follows is based largely on quotations and suffers from a lack of systematic treatment. Diamond contends that simply having women in the state houses does not necessarily serve a representational function; it is only when women are willing to identify with feminist issues and positions that substantive representation is achieved. Unfortunately, these conclusions are based on very small samples drawn from nonrepresentative states. Moreover, Diamond assumes that her activists come from competitive areas but she never establishes the linkage. While the questions asked in this section are extremely important, the author's analysis fails to provide sufficient evidence for her conclusions.

In summary, *Sex Roles in the State House* is a theoretically integrated book which asks an ambitious set of questions. The most problematic aspect of this work is that the analyses are at best uneven. For a substantial portion of the study the analysis falls short of answering some pertinent and provocative questions concerning the representation of women in politics. However, despite these shortcomings, the book is well worth reading. The discussion is well grounded in the party literature and brings a new slant to the interplay between structural and socialization factors in the recruitment of elites.

BARBARA G. FARAH

University of Michigan

Who Really Rules? New Haven and Community Power Reexamined. By G. William Domhoff. (New Brunswick, N.J.: Transaction Books, 1978. Pp. xiii + 189. $14.95, cloth; $6.95, paper.)

William Domhoff has written an important book. Although its most conspicuous characteristic is a Marxist perspective, the book makes contributions that are independent of its Marxist underpinnings. Domhoff provides an incisive critique (substantive and methodological) of Robert Dahl's *Who Governs?*, contributes importantly to the sociology of research, and offers a new slant on national/local political linkages. On top of this, the book is clearly and engagingly written.

In this book dedicated to Floyd Hunter, Domhoff makes a strong case for the view that the New Haven power structure is centered in the banks, the Chamber of Commerce, and Yale University. Fishing the same waters as Dahl, Polsby, and Wolfinger (indeed, drawing on research records that they made fully available), Domhoff casts a wider net and comes up with a much different catch. He finds, among other things, that a business-backed redevelopment agenda was already set when Richard Lee assumed office and that the executive-centered coalition was only a small segment of a much larger body of policy activity. He even argues that the tempo of urban renewal in New Haven was more a product of external forces than of mayoral skills.

Domhoff thus joins other recent writers in consigning elected officials to a minor role in policy formation. He depicts them as secondary figures, mainly doing the promotional work of popularizing the legitimizing proposals they did not initiate and over which they ultimately have little say. While Domhoff's view can be disputed, his evidence is formidable. All in all, his critique of the pluralist interpretation of New Haven is convincingly argued. However, as Domhoff shifts from critique to theoretical restatement, he is less persuasive. He plunges the reader into an icy bath of Marxism. The experience is stimulating but not addicting.

Domhoff shows that the business elite are well organized at the national and local level and that they actively exert influence in many ways, direct and indirect. Specifically he describes business involvement in post-World War II redevelopment policy and analyzes interconnections between business influence and "good government" reform. His general argument is that the governmental system is an instrument of the upper class, which (1) controls wealth and the corporate economy and (2) through this control dominates the media of communication and education and (3) also oversees a crucially important network of policy-planning organizations. Proposals not in the interest of the upper class thus have little opportunity to get on the decision makers' agenda. Domhoff maintains that the power structure in cities like New Haven "is part of the national ruling class" (p. 174).

Two difficulties plague Domhoff's argument. First he equates ruling class with upper class, and he maintains that "staying 'upper' is what 'ruling' is all about" (p. 13). But he isn't much more specific than that about what constitutes the interest of the ruling class. Since almost anything short of radical redistribution can be construed as keeping the upper class "upper," Domhoff's treatment of the interest of the ruling class is too inclusive to be helpful in analyzing political conflict and identifying class exploitation.

The second difficulty involves Domhoff's conception of systemic power and his failure to analyze in depth the relationship between the state and the corporate economy. While it is hard to disagree with his conclusion that national and local governments alike are promoters and protectors of major economic interests, his explanation of why this is the case is unconvincing.

Domhoff divides power into three levels: systemic, structural and situational. Instead of giving a full exposition of what each is and *how* it is involved in the general exercise of power, Domhoff provides three operationalizations. Who benefits is offered as an appropriate indicator of systemic power; who controls important institutional positions is an indicator of structural power; and who wins in decisional contests is an indicator of situational power. Other than a parenthetical notation that these three dimensions represent class, institutional, and decisional levels of power, Domhoff does not offer much conceptual clarification. His thoughts about how power is exercised can be inferred, however. The three aspects of power are like three levels of decision making: systemic power controls the broad agenda of policy making; structural power generates concrete policy proposals; and situational power resolves the details of specific policies.

The policy-planning network is thus central in Domhoff's view of how systemic power impinges on decision making. He is much entranced with the power of ideas, and puts forth the notion that there is a dominant ideology within which policy initiatives emerge and specific issues are resolved. His view of systemic power is thus one in which the upper class directly or indirectly controls key positions of authority, and uses these positions to maintain the dominant ideology and supply public officials with policy proposals. While Domhoff also calls attention to such factors as campaign contributions, he discusses wealth primarily as a means through which socializing and idea-generating instrumentalities are controlled.

Unfortunately Domhoff's fascination with ideology distracts him from other loci of business power: for a Marxist he pays relatively little attention to the underlying material situation (a stratified society in which wealth, including local government revenue, often comes through business activity). Since the main policy he examines is urban renewal, this is a particularly unfortunate neglect. Redevelopment is an area where the material interests of business and local officials converge even more than their ideas do—a towering fact of political life that should not be minimized. Possibly Mayor Lee's leadership in New Haven was skewed toward economic development, not because he was tied in with a ruling-class network, but because the city was materially dependent on major economic enterprises.

In his discussion of policy-planning organizations and local government structure, Domhoff covers a substantially different circumstance. He takes cognizance of the failure of many cities to adopt business-backed "good government" reforms, but he does so without drawing any conclusions. Government structure is an area where business and political interests diverge. It is also an area where ideological "dominance" did not produce compliance with a policy position behind which the business community placed a lot of support. Yet Domhoff steers away from any "therefore's." He simply does not subject ideology as a power factor to close scrutiny.

Ultimately, Domhoff's argument becomes that the upper-class auspices under which policy proposals emerge determine their later character. Possibly this is the case, but the reader is left with the unsatisfying feeling that Domhoff has not dug deep enough to explain the systemic interdependence between state and economy—that, in fact, he never gets farther than the institutional level of power. Arguments about ideological hegemony do not penetrate the understructure of political inequality. Maybe various social strata operate from such unequal footings that even in an ideologically diffuse setting *without a predetermined agenda,* policy would still take on a class character. It is unfortunate, then, that Domhoff did not pursue explanations of systemic power other than ideological dominance. Still, his empirical research is richer than the theoretical position he develops. Though some will find Domhoff's version of Marxism unappealing, he confronts us all with the need to understand dimensions of power beyond the personal and situational. Whatever quarrels one may pick with him over particular points, Domhoff has provided a much-needed reexamination of community power.

CLARENCE N. STONE

University of Maryland

Conflict and Crisis: The Presidency of Harry S. Truman, 1945–1948. By Robert J. Donovan. (New York: W. W. Norton, 1977. Pp. xvii + 473. $12.95.)

Of the ten main biographies of Harry S. Truman published to date, this is by far the best. McNaughton's and Hehmeyer's *Harry Truman—President* (1948) and Daniels' *The Man of Independence* (1950) were published while Truman was still in the White House, and their authors did not have access to many essential materials. Truman's own memoirs are still basic and anticipated many things that later appeared in the *Foreign Relations* volumes, but a frank autobiography is a rarity indeed and Truman naturally defended his record. Steinberg's *The Man from Missouri* (1962) suffers from a lack of documentation. A vast amount of material on Truman has become available in the last 15 years in the form of archives, memoirs, oral histories, and scholarly research. Phillips in his *Truman Presidency* (1966) used some of this but scorned the Truman Library holdings and his account of Truman's pre-presidential years is sketchy and inaccurate. Cochran's *Truman and Crisis Presidency* (1973) is the first revisionist biography. Margaret Truman's *Harry S. Truman* (1972) is what one would expect from an adoring daughter; it contains many errors and one wonders if she read the proofs. Miller's *Plain Speaking* (1973) is not real oral history but mostly fiction. Hamby's *Beyond the New Deal* (1973) is excellent, but it emphasizes liberalism and does not cover foreign policy.

Donovan's book has none of the drawbacks of the earlier biographies. It uses effectively the most recent available materials. It is judicious and fair-minded and is written in a lively style that brings out the drama of the years 1945–1948. As a White House correspondent during these years, Donovan was in a position to observe the reaction of Truman and his close associates to the crises as they occurred. He covers the literature thoroughly, he has footnoted his references carefully, and the index is well prepared. Donovan was ably assisted by Lawrence A. Yates in mining the holdings of the Truman Library. Thus he brings new material to the story of how Truman met the provocative Soviet postwar foreign policy, the conflicting demands of labor, business and the consumer during the troublesome years of demobilization and reconversion, the foreign needs for financial and technical assistance, the domestic dangers of inflation, and the attacks on the gains secured for the underprivileged under the New Deal and the Fair Deal. He is frank in depicting Truman's mistakes but he is not a revisionist.

Donovan's book is a corrective for the idolatry of the Truman revival of recent years as seen in Gallu's *"Give 'Em Hell Harry"–Reminiscences* (1975) and the downgrading Truman received during his presidency for his plebian manner, rough language, homey style and failure to be like his illustrious predecessor. How Donovan achieves this balance is shown by the following passage: "Suddenly handed the helm at a turning point in history, the former senator and small-town politician had been forced upon the journey unprepared, without being enabled by some miracle to shed a certain personal narrowness, ineptness, ignorance, impetuosity, and crudeness. Along the way, however, he was able to draw upon the resources of his own spirit, his own courage, humor, hope, good instincts, capacity to learn, willingness to face large challenges, and faith in the people and the American system of government. While he entrusted to other men like Marshall, Acheson, Byrnes, Forrestal, Clay, and MacArthur wide discretion in their fields and thus gave them a deep influence on the policies of his administration, Truman was always the president" (p. 436). Donovan points out Truman's ineptitude in his treatment of labor leaders, in the way he fired Ickes and Wallace, in shooting from the hip at press conferences, and in his handling of American-Soviet relations. On the other hand, he comments on Truman's preservation of New Deal gains, his innovative civil rights program, the Marshall Plan, the Berlin airlift, the struggle with the State Department over Israel, and the assumption of leadership in international affairs.

In his discussion of the CIA, Donovan relies heavily upon the findings of the Church committee which made its final report in 1976. Truman can hardly be blamed for things that happened long after he left the White House; the mistakes of Dulles, Eisenhower, Kennedy, Rusk, Johnson, and Nixon should not be laid at Truman's door. Newspaper writers show a lack of understanding of intelligence operations.

The 1948 election victory is labeled as "incredible." A closer look at political trends and at the mistakes of the pollsters would show that the result should not have been so unexpected. The press and the public were misled by inaccurate polling.

Donovan's book is a first-class piece of work and we should all look forward to his second volume on Truman as president in his own right.

HAROLD F. GOSNELL

Bethesda, Maryland

Felony Justice: An Organizational Analysis of Criminal Courts. By James Eisenstein and Herbert Jacob. (Boston: Little, Brown, 1977. Pp. viii + 322. $11.95, cloth; $5.95, paper.)

Once in a while a political science book appears which breaks sharply with established tradition, attacks conventional wisdom and displays courage and skill in the process. Such a book is *Felony Justice.* Public law, a staid field of study, can well afford the jolt provided by Eisenstein and Jacob, although many readers need to be warned not to discard old illusions for new half-truths. This is a very important book, but its implications are disturbing and possibly misleading.

The United States Justice Department estimates that about 90 percent of dispositions of criminal offenses charged are accounted for by plea bargaining. The authors of this book contend that "plea bargaining is not universally prevalent." In 1972 Jacob wrote that "the administration of justice in America is pockmarked by systematic discrimination against blacks, the poor, women, juveniles, and consumers." In this book he asserts, with his co-author, that in many ways "blacks are not treated worse than whites." In 1973 Jacob found that the fragmentation of functions and powers in the justice system was typical of the urban scene, and he described the system as "uncoordinated." In this book, a study of three criminal justice systems in Baltimore, Chicago, and Detroit during 1972–1973, the authors contend that judges, prosecutors and defense lawyers form "courtroom workgroups" which share common goals and seek to maintain group cohesion. Jacob seems to have changed his mind. Eisenstein also seems to have changed his mind on many of these issues, although not as much, judging by his earlier writings.

Jacob and Eisenstein directly challenge the conclusions of earlier scholars such as Blumberg, Packer, and Skolnick. The prevailing view of critics of the criminal justice system is that the due process model has been largely abandoned in the interest of efficient bureaucratic processing of suspects who are, to an extent, presumed guilty. The criminal justice system has been compared to a "confidence game" in which defendants are duped by the officials, including their own attorneys. This non-adversarial system is contrasted with the vigorous combat of civil trials. Most of these assumptions are rejected or questioned by Jacob and Eisenstein, who marshal formidable evidence in greater depth and detail than is usually found elsewhere in the very voluminous literature of the subject.

The book is essentially a methodological tour de force which will convince those who accept the method and the analytical techniques, and leave others unconvinced or annoyed. The courtroom work group concept is imposed upon the data because it permits generalization and hypotheses about the interaction of judges, prosecutors, and defendants. Extensive multivariate analysis is employed with an interesting array of statistical analyses, some of which does undermine traditional views about defendant characteristics as a determinant of the speed of dispositions.

The data base is fairly sound. The authors have traced the fate of 4500 defendants in three cities, a good-sized sample, yet the record-keeping in each city was very spotty. Also, measurement problems were encountered. Personal judgments about the quality of the evidence and about characteristics of the actions were made and included in the calculations. The authors omit, because of their definitions, the decision to arrest from the scope of their inquiry. They essentially exclude probation officers from their analysis, and they severely constrict reality by concentrating their attention upon the lawyer/actors who constitute the work groups.

The organizational analysis adopted by the authors supplies a surprising defense for the status quo in the criminal justice system. Because of the thin empirical base of most previous studies and the absence of an organizational framework, so-called issues of due process and fairness are regarded here as basically misinformed. Criminal justice reform is often misguided and emotional. If the authors are right, then plea bargaining isn't a bad practice. Defendants are generally treated fairly, regardless of income or race. Innocent people are not often railroaded to jail. Courts don't generally use plea bargaining to reduce heavy case loads. The authors may well be wrong, but it is up to each reader to appraise their evidence and their methods which may have distorted reality. Or perhaps Baltimore, Chicago, and Detroit are not a sufficient slice of reality.

The study rests on certain controversial assumptions. The criminal justice system operates "well" when there is a high level of interaction among the members of the work groups, according to the authors. Yet they find a vigorous, effective public defender in Detroit whose office really does help defendants and is isolated from other parts of the work group. Defendant-oriented lawyers may subvert the cooperative model of the study but they support the ideals of the adversarial system. In sum, the work-group model, while much more sophisticated than the bureaucratic model of previous writers, is still essentially statist and anti-individual. Besides that, the use of a work group model neglects the very real role played by police and prosecutors in changing and amplifying criminal legislation. In the long run repressive policies begin in the legislature, as do permissive policies. Reforms in sentencing and the movement towards decriminalization may have more to do with the shape of American criminal justice than cooperation in the courtroom. Lastly, due process and the adversarial process deserve more serious consideration before being discredited as antique notions. Due process isn't even in the index.

JAY A. SIGLER

Rutgers University

Land Use Control: Evaluating Economic and Political Effects. By David E. Ervin, James B. Fitch, R. Kenneth Godwin, W. Bruce Shepard, and Herbert H. Stoevener. (Cambridge, Mass.: Ballinger Publishing Co., J.B. Lippincott, 1977. Pp. xvii + 184. $17.50.)

This effort by a group of political scientists and economists presents a framework for analyzing local land use control measures and applies the framework to three measures—zoning, zoning with eminent domain (ZED), and transferable development rights (TDR). Although no new empirical research is presented, the synthesis of existing materials and the approach to evaluation should prove valuable to local planners and to policy analysts.

The authors examine each of the three mechanisms from the standpoint of five criteria, the first three of which relate to economic efficiency: (1) reduction of negative external effects, (2) the provision of public goods, e.g., open space, (3) reduction of the costs of providing public services such as roads and schools, (4) distributional effects, and (5) political feasibility. They first review a number of the limitations of zoning, including the inequities imposed on landowners whose property is designated for open space, its limited ability to reduce negative externalities, and the frequent erosion of land use plans through variances and conditional use permits. In order to deal with such problems efficiently and equitably, a number of methods have been suggested in recent years—including zoning by eminent domain and transferable development rights—whereby those whose land is deprived of its development potential would be compensated by those who benefit from such policies. The authors argue persuasively, however, that each of these new mechanisms has considerable problems of its own.

The substantive analysis is, however, probably less important than the study's evaluative framework. The framework emphasizes systemwide efficiency but is broadened to include distributive impacts and political feasibility. Still, the economists' emphasis on efficiency tends to dominate the analysis (especially of zoning) and leads to some dubious assertions concerning actual behavior. For example, the chapter on efficiency criteria sometimes argues that considerations of efficiency are paramount to local decision makers—an argument at direct odds with the chapter on political feasibility, which contends—correctly, we think—that local officials are primarily concerned with the distributive impacts on important constituent groups. Likewise, the discussion of zoning contends that its principal rationale has been the reduction of externalities, with the protection of the socioeconomic homogeneity of the neighborhood and property values a secondary consideration. We suggest that the order be reversed. In addition to these highly questionable statements about behavior, the implication that efficiency benefits and costs can be adequately measured—a requirement, if efficiency is the decision criterion—is open to question.

The equity section focuses primarily on methodological and data problems of estimating the impacts of changing land and housing prices on different income and age groups and different categories of landowners and renters. Although other dimensions of equity and other groups could have been considered, those covered are clearly the most important to the political process.

The final component of the framework deals with political feasibility and proposes an approach similar to those suggested by Graham Allison and Harold Luft. First, Ervin et al. estimate the incidence of benefits and costs of a specific proposal on various groups and then they predict the likely response of each group in trying to influence the decision process. The discussion of political participation is particularly outstanding, both in terms of the explicitness and sophistication of the general model and the application to various groups of actors for each of the three land use control mechanisms. Estimating the distributional impacts and participation levels is, however, a rather problematic task, and the authors correctly emphasize the negative effect of such uncertainty on the adoption of innovative programs such as zoning by eminent domain and transferable development rights. Nevertheless, it would have been instructive if the authors had tested their political feasibility framework by analyzing why approaches such as ZED and TDR have been proposed several times but enacted very seldom, or had predicted the situations in which such innovations are most likely to be enacted and/or implemented.

These criticisms of the general framework are not meant to detract from the book's excellent analysis of the three land use control measures. The discussions of transferable development rights and zoning by eminent domain are especially good and quite readable, in part because considerations of distributive impacts and political feasibility are given equal treatment with efficiency effects. In balance, then, the book makes a significant contribution to the analysis of land use control policy.

PAUL SABATIER

S. I. SCHWARTZ

University of California, Davis

Taxation and Political Change in the Young Nation, 1781–1833. By Dall W. Forsythe. (New York: Columbia University Press, 1977. Pp. 167. $15.00.)

For many years, political change and development have been the major foci of the study of both European and non-Western polities. Political development in the United States, by contrast, has received relatively little scholarly attention. Only a small number of modern studies even touch upon historical changes in the shape of American political institutions and patterns of public policy. It has often been noted that in its early stages the study of

American political behavior seemed to assume a timeless and changeless political process. Easton, for example, suggested that while there existed "always some change," change was not generically different from continuity. The difference between the two was primarily a matter of the rate of change. This argument, of course, obscured the distinction between change in the sense of trend and change in the sense of discontinuity. Moreover, what Forsythe terms an overextension and oversimplification of the arguments put forward by Hartz in his influential *Liberal Tradition in America,* appeared to support the proposition that America was a completely unique case. Overextension of the Hartz thesis suggested that political development had never taken place in the United States.

To an older generation of scholars, trained in law and public administration, American politics was not "all of a piece." The scholarly works of Edward Corwin and Leonard White, among others, point to periods of constitutional or, as Forsythe would have it, "regime change," during which public policy, political institutions and the political process itself were substantially altered. Corwin's examination of judicial doctrines affecting property rights, for example, indicates the occurrence of major turning points, with profound political and economic implications, in the history of the relationship between private rights and public authority. During the early years of the behavioral revolution in the study of American politics, studies like Corwin's were often conceived to be examples of sterile legalism, and did little to disturb the prevailing view of the seamlessness of the American political process.

V. O. Key's "Theory of Critical Elections," which identified periodic alterations in the structure of mass political cleavage in the United States, was among the first empirical studies to demonstrate the existence of important discontinuities in American political history. Burnham and others, inspired by Key, have since attempted to assess some of the institutional implications and correlates of these discontinuities in mass political behavior. Yet, with the exception of these and a small number of other important studies—Polsby's provocative work on the House of Representatives is one example—the study of institutional development in the United States has continued to be neglected.

In *Taxation and Political Change,* Forsythe attempts to probe some of the most critical questions of American political development. Forsythe argues that in the United States, as elsewhere, the essential precondition for institutional development was the creation and expansion of the national government's capacity to extract revenues from its citizenry. By examining the formulation and implementation of tax policies during the early years of the American republic, Forsythe traces a crucial element of the history of state-building in America. He focuses first on Hamilton's fiscal system, which had as an explicit goal development of the central state apparatus, and then proceeds to discuss the early history of American internal taxes and tariff policies. Though students of American history are familiar with all these measures, when conceived as case instances of state-building, they take on new importance. In particular, the sometimes puzzling history of conflict over taxes and tariffs becomes considerably more meaningful when seen as sometimes involving the fundamental issue of the central state's boundaries—the potential scope of the state's authority in American society.

Forsythe does not devote sufficient attention to the antecedent and boundary conditions for state-building in the United States. What factors and circumstances, for example, seem to induce and to inhibit expansion of the government's capacity to tax? He, nevertheless, helps to outline a number of important ways of examining American institutional development and relating it to changes in mass political behavior and public policy.

All in all, *Taxation and Political Change* represents a useful step towards understanding political development in the United States, and a step away from a conception of America as a unique unchanging polity.

BENJAMIN GINSBERG

Cornell University

Constitutional Counterrevolution? The Warren Court and the Burger Court: Judicial Policy Making in Modern America. By Richard Y. Funston. (New York: Schenckman Publishing Company, Halsted Press, John Wiley, 1977. Pp. xiii + 399. $17.50.)

In this substantial analysis and comparison of the Warren and Burger Courts, Funston concentrates on the three areas that Chief Justice Warren himself selected as the most important aspects of the Court's work during his tenure—race relations, legislative apportionment, and criminal defendants' rights. There are long chapters on each of these themes, plus shorter chapters on obscenity ("Nine Old Men and a Few Dirty Books"), religion ("Much Ado About Nothing"), and the retroactivity problem ("Of Chimeras and Court and Consternations"). Concluding chapters appraise separately the Warren and Burger Courts.

Funston's answer to the question posed in his title is that a constitutional counterrevolution has not occurred. The change from Warren to Burger has not been "as great as was anticipated; except in the area of the criminal law and the censorship of obscenity, the work of the Warren Court remains intact; and even in those areas the Burger Court's approach has been one of qualification rather than of outright reversal" (p. 372). In spite of false impressions created largely by the media, the differences between the two courts "have been more matters of rhetoric than of reality, of degree rather than of substance" (p. 351). This is, in part, because the Burger Court's judicial conservatism—adherence to *stare decisis*—has been more powerful than its political conservatism. There has been "a restrained, though unenthusiastic, continuity with Warren Court precedent" (p. 350). And partly it is because only Rehnquist and Burger have an "uncompromising philosophic hostility" toward the positions of the Warren Court.

In spite of the continuity, Funston does find some new elements in Burger Court decision making. One is the enhanced value given to privacy, an idea whose time has come. Another is the greater deference accorded legislative judgments, along with a willingness to shift the burden of decision down to the local level, which contrasts with the Warren Court's "nationalization" of American politics and problem solving. In fact, Funston concludes that "the most pronounced characteristic of Burger Court decision-making has come to be deference to the judgment of other governmental entities" (p. 372). The author also notes the Burger Court's campaign to limit access to the federal courts, including resurrection of the abstention doctrine, limitation of federal habeas corpus relief and the overbreadth doctrine, and increased concern about standing.

Funston does not have much rapport with judicial behavioralism, but of necessity he takes some account of judicial blocs. Particularly during the earlier years of the Burger Court, Stewart and White were the swing justices. Of the two, Funston regards Stewart, with his ad hoc "I know it when I see it" approach, as the more influential, and he is not happy about this; "a Court which mirrors [Stewart's] approach has been a Court adrift" (p. 359).

A familiar complaint of the purists was that the Warren Court's "craftsmanship" was poor. Funston does not find that the Burger Court has done much better in providing reasoned and reasonable explanations for its policy choices. Its decisions are not uniformly mediocre, but they are uneven. An apologia is offered; the Burger Court has permitted itself to be trapped by working with Warren Court precedents that were philosophically fragile, thus introducing into its own opinions the intellectual confusion of the Warren Court.

Funston considers that Burger has on occasion suggested a "Madisonian mentality," but again, "when the limits of the judicial process have felt too confining, the Chief Justice has manifested a penchant for statute drafting, advising the nation's legislators on how to control pornography and secure the overthrow of the exclusionary rule" (p. 357). He regards Burger as closer to Warren's jurisprudence and talents than to those of Frankfurter or Jackson, and this is not meant as praise.

Funston suffers a scholarly compulsion to leave no stone unturned or case uncited, and the hypothetical lay reader, for whom the author suggests the book was written, may bog down in the 80 pages and 250 footnotes of the criminal rights chapter. But the author does succeed in his goal of making Supreme Court decisions interesting and understandable. The accounts of cases are lively, often with extra-case incidents and consequences. Funston does not hesitate to pass judgments, mostly critical. His orientation is anti-activist, pro-"passive virtues," redolent of Bickel, Kurland, and McCloskey. He thinks it would be poetic justice if the Burger Court should reverse the Warren Court's policies by using Warren Court methods. "Warren Court precedent, after all, is far from firmly entrenched and would seem to be ripe for eradication and removal by a Court willing to copy the Warren Court's own blithe regard for *stare decisis*" (p. 364).

C. HERMAN PRITCHETT

University of California, Santa Barbara

A Portrait of Marginality: The Political Behavior of the American Woman. Edited by Marianne Githens and Jewel L. Prestage. (New York: Longman, Inc., 1977. Pp. xvii + 428. $6.95, paper.)

This collection of essays edited by Githens and Prestage on female political behavior in America is a welcome addition to the sparse literature in the field. The articles are organized into sections which explore socialization, recruitment, participation and performance. Special attention is given to the role of black women. Of the 22 articles included, half were originally given as papers at national and regional meetings, 7 have appeared in journals or books, and only 4 were apparently written especially for this volume. Thus the reader must rely on the introductory sections and the conclusion, which are succinct and well written, to provide the unifying theme: the concept of marginality. A major weakness of the book is that the articles rarely focus directly on this issue, much less in the analytical terms introduced by the editors, so what might become a useful theoretical model is never fully developed.

For Githens and Prestage, both role conflict theory, which explains the low level of female participation as elites to be the result of "tensions between ascribed and achieved status," and the Marxist view that underrepresentation is a result of capitalism, are inadequate because they are static. They ignore the "dynamic of interaction" between men and women. They use Ralph Turner's definition of marginality (in *The Social Context of Ambition*) as the condition of existing "on the borders of two groups" that place "contradictory demands" on the individual, as seen in the experience of the Jew seeking to become assimilated into the Gentile world (pp. 5–7). Paradoxically, marginality produces both innovativeness and anomie and is often accompanied by hypersensitivity, irritability and lack of ease in personal relationships. In applying this model to women politicians, Githens and Prestage argue that:

> women who seek to enter the male-dominated political elite reject . . . at least some of the values and norms of most women. Like the Jew, the political woman no longer feels comfortable with non-political women; she has problems engaging in informal conversation and in acknowledging other women's evaluations as criteria for her own self esteem. On the other hand, the politician group, where she wishes to establish contact, displays reticence in accepting her (p. 7).

Women lack a supportive reference group and do not have appropriate opportunities to learn the norms and values of male politicians through anticipatory socialization.

In addition to the difficulty of making the case that women politicians are hypersensitive or irritable, it is ironic that one of the major conclusions that emerges from the book is that male/female differences in political participation and performance are not nearly as important as we have come to believe. Stucker finds that the gap between male and female voting participation "has virtually disappeared" (p. 278), and Lansing notes that this is also true for blacks (p. 392). Orum, Cohen, Grasmuck and Orum find no significant sex differences in political cognition and expression among Illinois school children (p. 29), and Gehlen concludes that, from her study of the characteristics of men and women in Congress, it would make "very little" difference if more women were elected (p. 317).

Where there are differences, and the severe under-representation of women keeps us looking for that obvious "objective" explanation to emerge from the data, the contributors offer evidence of role conflict but few symptoms of marginality. Constantini and Craik find that the woman politician, "like the wife, tends to specialize in expressive functions" and "is relegated to, or relegates herself to, a supportive role of more or less selfless service to her family or party" (p. 239), and many articles note the incompatibility between child rearing and office holding.

Like role conflict, the theory of marginality blames problems of adjustment and ambition on the ambivalence of the female aspirant. Yet it can be argued that women politicians respond in ways that do not fully fit the male norm not out of ambivalence but out of fear. A study of 1972 convention delegates by Lynn and Flora hypothesizes that female office holding is "deviant," and that "systemic punishment that results from deviant behavior leads to a preference more compatible with role expectations" (p. 139). Lee found that three-fourths of her sample of local women politicians felt that "not being accepted by men" was a major problem; 69 percent of the male politicians they were working with agreed that "most men would prefer women to participate in politics in ways other than running for office" (p. 132). Iglitzin shows how the welfare system regulates the role, sexuality, labor force participation and self-image of its female clients to conform to traditional mores and to facilitate exploitation.

It would seem that a theory purporting to reflect the "dynamic of interaction" would have to look at *interactions,* e.g., male responses to female ambition, and, to be thorough, should examine males as well for symptoms of ambivalence and hypersensitivity. Marginality reflects only one side of the equation, the other side of which is male response to real or threatened loss of power. However, focusing on theoretical issues I have slighted this book's important substantive contributions. The fact that marginality is not yet compelling as a theory of female under-representation does not diminish the importance, timeliness and usefulness of this essential anthology.

JANE S. JAQUETTE

Occidental College

Evolution of the Modern Presidency: A Bibliographical Survey. By Fred I. Greenstein, Larry Berman and Alvin S. Felzenberg, with Doris Lidtke. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1977. Pp. xv + 2504. $4.75, paper.)

Studying the Presidency: Results, Needs, and Resources. By Hugh Heclo. (Naugatuck, Conn.: Ford Foundation, 1977. Pp. 53. $2.95, paper.)

Here are two boons for teachers and graduate students of public administration and more particularly for students of the presidency. Hugh Heclo, whose recent Brookings book, *A Government of Strangers: Executive Politics in Washington,* might serve as a text on the subject, has distilled some advice for the Ford Foundation on financial support for research in that field. The Foundation, not waiting for a Freedom of Information Act, has published Heclo's advice. This shows that we can count on market incentives and the Government Printing Office to continue to supply a great mass of materials on many aspects of the presidency and its incumbents without Foundation support, but serious deficiencies nevertheless persist. As Heclo says in *Studying the Presidency:* "What is most needed in current Presidential research is not history for its own sake but studies that contribute to our understanding of how central government performs and how it might perform better" (p. 38). More specifically, "It should: (1) concentrate on processes and behavior in the White House and the Executive Office of the President; (2) describe, explain, and evaluate experiences in more than one administration; (3) make use of primary materials and documentation to be found in Presidential libraries and elsewhere; (4) concentrate on recurring problems that are likely to be relevant for future administrations" (p. 52). He notes two material obstacles in addition to problems of declassification: that the Presidential libraries are geographically dispersed, inconveniently for his third and fourth criteria; and that the kernels of significant, perhaps indispensable evidence they contain are buried in a suffocating mass of irrelevant and routine materials.

Heclo's homilies apart, this pamphlet-sized essay should be welcome for its orderly topical review and judicious selection and appraisal of a limited number of references under five general headings: the man, public politics, Washington politics, executive politics, and didactic reviews. These bring him to focus on the problem-solving institutions and processes in the Executive Office of the President as the principal target for needed research. Heclo has apparently canvassed the array of unpublished dissertations, completed and in progress, and his selections include some of these.

Brookings' emerging rival, the American Enterprise Institute for Public Policy Research, has published a far more compendious bibliography in the same general field, a by-product and logistic support of the substantive work of Fred Greenstein and some of his associates at Princeton, now put at the disposal of other toilers in the vineyard. (It began as a compilation by Joan Jurale, reference librarian at Wesleyan University, Greenstein's previous base.) Its 2504 entries are arranged according to a much more detailed outline of the subject, partly on encyclopedic and partly on chronological principles. Of its 21 chapters, seven are devoted successively to the presidencies from Franklin Roosevelt's through Gerald Ford's. Chapters 16 and 17 embrace the subjects of Heclo's concerns. Greenstein's preface includes a short exposition of some distinguishing characteristics of the institution that justify his limiting his scope by the term "modern."

The collection sticks to published materials. Chapter 2, prepared by James Fetzer and making up about five percent of the total, consists of selected government documents, arranged on the same plan as the rest of the book. Periodical articles are listed as well as books. Some but by no means all of the entries are briefly annotated.

As Greenstein notes (p. xiii), "there is an inevitable arbitrariness in making the selections." For instance, I missed Bryce Harlow's first-hand account of the establishment and operation of the congressional liaison office in the White House, buried obscurely in the *Center House Bulletin* (Winter 1974) and the Budget Bureau's anonymous *United States at War* (1947), recounting the organizational improvisations, many of them formally parts of the Executive Office of the President, or the Office of Emergency Management therein, devised to cope with new and urgent functions during World War II.

Heclo furnishes an admirable reading list suitable for a course or for preparation for a general examination, along with his pointers toward further fruitful research. Greenstein's bibliography would swamp a reader; its utility is for the researcher with a topic already in mind. On the heels of these two a third list of some 250 items has just appeared: "Presidential Interactions: An Annotated Bibliography," by Steven A. Shull, in *Presidential Studies Quarterly* (Winter 1978).

HARVEY C. MANSFIELD, SR.

Columbia University

The American Presidency: The Dilemmas of Shared Power and Divided Government. By Ernest S. Griffith. (New York: New York University Press, 1976. Pp. x + 241. $13.50, cloth; $4.95, paper.)

At the risk of beginning on what sounds like a negative note, this is the "umpteenth" book published with the title "The American Presidency," and does not carry us much beyond its predecessors. To be fair, the author had the difficult assignment of distilling a volume out of the record of a conference on the institutional presidency sponsored by the National Academy of Public Administration in 1974. The book inevitably bears the mark of its origin in what was no doubt an often stimulating, insightful, but nonetheless discursive kind of enterprise. Conferences of this sort may draw on scholarship and research, they may stimulate further work in the future, but they themselves rarely produce conclusions or analyses which significantly advance knowledge.

This *American Presidency,* not surprisingly, is not really a book in the sense of being an integrated and analytical treatment of the subject. Again, to be fair to Griffith, the literature on the presidency generally leaves much to be desired when compared with the work that has been done recently on Congress, for example. This volume is no worse, and in some ways better and more useful than much that has has been written on the subject. All too often such books are descriptive, broadbrush, prescriptive, and remarkably uniform. What is conspicuously lacking—though with notable exceptions—are studies of the office which *do* attempt to probe, to gather and analyze data, and thus to contribute new insights. Efforts to treat *the* presidency in toto, almost certainly defeat these objectives.

Griffith's book is of the same general, hortatory kind. Only in its timing does it contribute something not already available in other "American Presidency" volumes. I can only say "Amen" to the author's assertion: "The conference and recent writings regarding the Presidency have been understandably—but unfortunately—preoccupied and somewhat distorted in their deliberations and emphases by Vietnam and Watergate" (p. vii). Griffith tries to avoid such distortions, and in my view, does so quite successfully.

Academe and the liberal establishment saw Watergate—rightly, indeed—as the complete justification of their long-standing contempt for Richard Nixon. Griffith, while hardly a Nixon apologist, tries to set some of the accomplishments and proposals of the Nixon years in a more balanced perspective. He cites the quite striking foreign policy achievements of the Nixon-Kissinger team. On the domestic side, he discusses the establishment of the Domestic Council, the effort to exercise greater control over the administrative establishment from the White House, and the massive departmental reorganization plan submitted to Congress (fruitlessly, as it turned out).

The Domestic Council, modeled on the National Security Council, clearly filled a need. The White House had to have some more formal instrument for dealing with the interjurisdictional controversies of our incredibly pluralistic administrative structure, and for policy and program planning. Nixon's other efforts to centralize control in the White House were probably undertaken for the wrong reasons, if they were not actually dangerous. Yet it is hardly debatable that only the president can, to borrow an old saw which Griffith quotes: "make a mesh of things." That to do so the president needs to exert power through staff in the White House seems equally evident.

Rationalizing the departmental structure into new "goal-oriented" units dealing with Human Resources, Natural Resources, Community Development, and Economic Affairs as Nixon proposed would make all kinds of sense, except political! Clientele ties would be weakened, cozy policy subsystems (whirlpools, Griffith calls them) would be disrupted, and the president would have a sensible structure through which to exercise control (p. 196 ff.). Waterfate intervened, though the plan never would have moved, even if there had been no scandal. Griffith sums up these efforts: "Nixon at least raised some basic issues. Quite possibly all future discussions of any consequence on our federal government as a whole will now have to take them into account" (p. 201). This is a useful corrective to current undiscriminating anti-Nixonism.

However, the book remains a rambling, if at times insightful, review of the problems and critiques of the American national political system. Frequently one must infer the relationship of the problems raised to the presidency.

The level of discussion is vastly uneven. Many of the points made are sophisticated, but then one encounters, on the other hand, an avowedly elementary discussion of Congress' "organization, leadership and motivations" (p. 93).

In sum, taken for what it is, and its origins understood, this is a useful book. It breaks no new analytical ground, but it does contribute to the necessary evaluation of the often simplistic, not to say hysterical, Watergate-inspired revisionist discussions of the presidency.

ELMER E. CORNWELL, JR.

Brown University

Mass Society and the Extension of Welfare, 1960–1970. By Kirsten A. Grønbjerg. (Chicago: University of Chicago Press, 1977. Pp. 266. $16.00.)

The study of American public policy has certain ongoing characteristics–increasingly comparative, longitudinal, quantitative and relevant to general social and political theory. Kirsten Grønbjerg's book on changes in state and federal welfare policy is a new and excellent example of the field both for nonspecialists interested in assessing progress in the field and for specialists in state politics and policy. Like all good theses, Grønbjerg's is deceptively simple yet potentially dramatic. It may be summarized by the following question: Can the persistence–in fact, the expansion–of welfare "incidence and welfare payments" be satisfactorily accounted for by the explanation of its origin, namely, the response to changing social and economic conditions? The social and economic explanation Grønbjerg labels the "social stratification" explanation, which is that welfare eligibility standards, the number of people on welfare, and monetary support are largely responses to such variables as poverty, unemployment and the number of broken families. Such an explanation runs up against several problems. First of all, in simple longitudinal terms, the national statistics on welfare numbers, poverty, unemployment, divorce and desertion rates are only vaguely related. That is, while the nation gets richer and richer, the welfare rolls get larger and larger. And even at the state level, obvious indicators of Grønbjerg's social stratification model such as the numbers below poverty levels, the unemployment rate, the number of broken families and so on are inperfectly related to the size of the welfare rolls and payments.

The federal nature of the welfare system and the enormous variations among the states indicates that no single national welfare polity exists. The nation does not respond uniformly to the problem of economic change and dependency, but rather with discordant and jerky responses. The power in Grønbjerg's analysis rests in the nature of her alternative "mass society" explanation which accounts for both of these differences–variations in the longitudinal national response as well as the cross-sectional 50-state variation. According to Grønbjerg, the contemporary expansion in welfare rolls and payments comes about from "a glacial and uneven shift toward the democratization of human rights" (p. 2). Drawing upon Edward Shils' *Center and Periphery* and T. H. Marshall's *Class, Citizenship and Social Development,* Grønbjerg claims that welfare has assumed the status of a right of citizenship. However, the recognition and actualization of that right is unevenly spread and rapidly granted across the nation. This is not simply a case of which states have the highest welfare payments, for "the extension of social rights is greatest when the largest number of people are provided with assistance and the standards of support are the highest" (p. 24).

Grønbjerg relies on data from the 1960 and 1970 censuses, neatly bracketing the most explosive and controversial growth period in the AFDC program, to test formally for the shift from the "social stratification model" to the "mass society model." She discards one explanation for the change in welfare rolls (nearly an 80 percent gain over the 1960–70 period) by coding state-by-state policy on eligibility requirements and constructing a "total eligibility" score for each state. That index is essentially unrelated to the size of the welfare rolls. States with more liberal eligibility standards are not necessarily those with massive welfare rolls. By 1970, "cost standard" for enrollment in AFDC and the average state AFDC payments were also unrelated to the proportion of families in the program. "The major importance of public assistance policies consequently may lie not so much in being able to control the size of the welfare rolls . . . as in reflecting public concerns about poverty and dependency in the states. As a result, state legislation on public welfare is largely a symbolic crusade . . . episodic, isolated occasions. . ." (p. 54). Grønbjerg points out that a by-product of the increasing range and complexity of federal welfare programs and regulations is that state politicians can have it both ways–progressive welfare programs and policies, together with symbolic, hostile crusades against welfare.

The statistical test for the major hypotheses is a cleanly presented and well-informed stepwise regression analysis. Chapter 6 examines the most powerful set of determinants of the 1960 interstate variation in welfare rolls. The result is that the determinants of AFDC as the public policy "response to deprivation and need in the various states was primarily in the form of the stratification approach, reflecting objective conditions of need" (p. 93). That is, the most powerful determinants of the size of the 1960 AFDC rolls in the states were the standard poverty and income statistics—"percent of population in broken families," underemployment, nonworker-worker ratio, and percent total inmigration between 1955 and 1960. Only the fourth variable in the step-wise regression, percent voter participation, did not qualify as an obvious indicator of the stratification model.

While it would be nice to say that the regression analysis for the 1970 AFDC statistics or the regression analysis of the difference between the 1960 and 1970 AFDC rolls established indicators of the mass society model as prime determinants, such is not the case. On balance, Grønbjerg discovers that shifts did occur in the relative power of the two models. She discovered that

> urbanized and industrialized states with well-educated population, high degrees of citizen participation in politics (voter participation), large upper white-collar occupational groups, and a willingness to tax themselves [are those] most willing to extend public assistance to a larger proportion of the population than could be expected on the basis of personal need alone. Such states, then, would have more liberal interpretations of economic need and would thus assume the responsibility for broader social needs (p. 155).

Grønbjerg closes her work with a discussion of some likely causes of the expansion in the definition of citizenship and barriers to its full realization. In summary, this is a very impressive volume written in a forthright and interesting manner. The thesis is well stated, clearly presented, and analyzed in a sophisticated quantitative style.

RICHARD WINTERS

Dartmouth College

The Transformation of American Law, 1780–1860. By Morton J. Horwitz. (Cambridge: Harvard University Press, 1977. Pp. xvii + 356. $16.50.)

This book is an impressive achievement. Written by a Harvard law professor who obtained a Ph.D. in political science before thinking the better of the discipline and acquiring a J.D., it also testifies to a disconcerting circumstance. As was not true a generation ago, only a small share of the really interesting work linking law, society, and polity is being done in political science. Of late, lawyers and legal historians seem to be telling us most of the significant things.

The Transformation of American Law should be read closely by political scientists who specialize in public law and judicial process. But why—when it focuses on the path of American law between the Revolution and the Civil War? The first reason is that much political science teaching and research in the area are anchored in an assumption about the evolution of judicial roles which turns out not to be viable. It is the assumption, John Marshall and "nation building" aside (and perhaps also Roger Taney and "slavery maintenance" aside), that the American judicial tradition admits of a rough dichotomy between pre-legal-realist, nineteenth-century judges who believed they merely, and formalistically, applied rules, and legal-realist jurists who realized the social instrumentalism inherent in their roles as "rule makers." Horwitz argues that as early as the Adams-Jefferson era judges set out, with a deliberateness of purpose at least equal to that of any good Warren Court activist, to reshape the main structures of American law. Of course, Horwitz is not the first to say this. The names of William Nelson and, earlier, Karl Llewellyn spring to mind. But after Horwitz's very full development of this theme, there is little room left for arguing that judicial lawmaking is a twentieth-century novelty. Rather, if there is argument, it must turn on when American judges ever thought they did anything else. (Some of Horwitz's—and Nelson's—critics would push the beginnings of judicial instrumentalism further back into the eighteenth century than 1780. Others see less formalism in the late nineteenth century.)

The second reason lies in the book's telling demonstration that the great bulk of the changes wrought in the American legal system did not occur in the public law sector, toward which political scientists have traditionally directed their concerns. Rather, the most important "transformations" came about in the private law sectors—to give a few examples, in the erosion of civil jury power; in the shift from conceiving of the law as enforcing "objective," equitable business relationships among people to thinking of it as enforcing a "subjective," will theory of contracts; in the movement away from property rights identified with natural, quiet and agrarian uses to property rights thought legitimate insofar as the uses furthered industrial development.

One benefit of Horwitz's analysis is that, if constitutional doctrine is embedded in the evolution of private law, some of the apparent anomalies of past Supreme Court Justices' positions dissolve, or appear in quite a different light (e.g., Story's *Charles River Bridge* dissent, and his aims in *Swift* v. *Tyson*).

Another benefit, especially for those interested in current issues of public policy and law such as environmental protection, consumer protection and the law of negligence, is the curtain lifted on earlier, eighteenth-century conceptions of juridical purposes and processes that have much in common with arguments and values being pushed in "post-industrial" litigation today. Also, and particularly for those of a radical persuasion, there is the benefit of having, at last, a sophisticated treatment of how American law came to be "exploitative."

To say this is to point to the book's two chief limitations—its almost exclusive focus on the law's transformations in the North Atlantic states, and its "driving explanation" of judicial behavior.

With respect to the first, Horwitz can insist that the northeast is where the transformations crucial to his concerns occurred. Nonetheless, something more about the development (or nondevelopment) of southern and (middle) western law other than the book's occasional sallies into South Carolina and Virginia jurisprudence might have proved helpful. There is, thus, no discussion of the connection between the law of slavery and the law of commercial development. Yet such a discussion might, in the contrast, have made North Atlantic developments appear somewhat less exploitative. For—and this reaches the second limitation—what explains for Horwitz the transformations is an alliance between judges and lawyers which, forged in the early years of the Republic, managed to put most of the costs of development on those who were not entrepreneurs.

Horwitz's argumentation on this score is intriguing. There are fascinating chapters and sections on, for instance, the shift from strict liability to negligence, on the development of the fellow servant rule, on Massachusetts litigation over de facto appropriations by mill owners of adjacent property owners' land and water use rights—all amounting to judicial legislation that put substantial developmental costs on workers and agrarian property owners, not to mention the general public. Yet, while the upshot may have been regressive income redistribution (as Horwitz argues), two points remain less than well proven. One is whether in fact only a few prospered while the bulk of the citizenry paid the costs of development, or whether a few got more than their fair share but many others, though getting less, still got more than they would have without industrialization. The other is whether the judges and their lawyer allies were quite so deliberately engaged in plotting the upshot as Horwitz frequently seems to imply. Judicial motivation, too often, seems to be deduced from the distributive consequences of case law, rather than induced from direct evidence of an alliance.

These caveats aside, *The Transformation of American Law* will be a work to be reckoned with by all those interested in the development of the American legal system.

A. E. KEIR NASH

University of California, Santa Barbara

Pragmatism, Statesmanship, and the Supreme Court. By Gary J. Jacobsohn. (Ithaca, N.Y.: Cornell University Press, 1977. Pp. 214. $12.50.)

This book is, primarily, an argument on behalf of judicial statesmanship, which Gary Jacobsohn defines as the judicial adaptation of enduring constitutional principles to changing factual situations. It is, secondarily, a book about pragmatism, a school of thought whose premises, Jacobsohn argues, are incompatible with statesmanship, and whose influence has been largely responsible for its neglect during this century.

Jacobsohn devotes the first part of his analysis to a discussion of pragmatism, drawing from the work of Dewey, Pound, and Cardozo. This is a prelude to a contrast between pragmatism and the prevailing natural rights principles which pragmatism sought to replace. Pragmatism, in rejecting natural rights, denigrated the capacity of abstract reason to guide social action, and turned instead to a primary reliance on experience, experiment, and empirical verification. It distrusted abstract principle in general, and sought to judge the worth of particular action through an examination of its social consequences. In plact of a conception of a fixed human nature, and a set of fixed human ends derivable from nature and accessible to reason, which had characterized the natural rights understanding, pragmatism adopted a faith in evolutionary progress and in an open-ended human growth. With the disappearance of fixed ends, and of fixed constitutional principles embodying those ends, the judicial task was transformed from that of upholder of principle to one of social engineer.

Jacobsohn's argument for judicial statesmanship rests on the assertion of the continued vitality of natural rights principles. In making this argument he is not asking for adjudication in disregard of changing social needs. It is, he asserts, necessary neither inflexibly to apply absolute principle to new circumstances, as did Justice Field, for example, nor to reject all principle, as did pragmatism. Rather, the statesman's task is to maintain the integrity of constitutional principle, while adapting it to meet changing needs. Justice Harlan's opinion in *Lochner* v. *New York*, and Justice Hughes' in *Home Building and Loan Association* v. *Blaisdell*, are offered as examples of the statesmanlike application of the principles of freedom of contract, and of obligation of contract, and are contrasted to both Justice Field's unyielding application of absolute principle, and Justice Holmes' pragmatic abandonment of principle. There is also a lengthy discussion of the reapportionment opinions in which it is argued that the majority's requirement of equal population districts is, contrary to first impression, actually an example of the pragmatic rejection of constitutional principles of democratic representation. Justice Stewart's opinion, which recognized the complexity of the constitutional principles, while seeking to remedy the grossest inequalities, is offered as an example of the statesmanlike approach to this problem.

There is much of value in this argument that takes seriously the possibility of statesmanship, the importance of principle for the guidance of human action, the centrality of reason in the derivation and maintenance of these principles, and the need for prudent application of principle to diverse facts. There is also much value in an analysis of the Supreme Court which emphasizes its unique opportunity to provide sensitive political leadership in American political life.

These positive contributions are marred, however, by certain limitations. The most serious is Jacobsohn's inadequate treatment of political principle. Contemporary political life is characterized by the extreme difficulty of recognizing and maintaining fundamental political principle. Yet, Jacobsohn asks us, without extensive supporting argument, simply to accept natural rights principles as enduring ones, and the Constitution as the embodiment of political wisdom. His welcome insistence on the need to adapt these principles to changing circumstances certainly does not constitute an argument on behalf of the viability of the principles themselves. This inadequate defense of natural rights is reflected in the discussion of pragmatism. Pragmatism did not arise solely in response to the excesses of natural rights advocates, as Jacobsohn suggests, but, simultaneously, in fundamental disagreement with the natural rights principles themselves. If this case for judicial statesmanship is to be persuasive, there has to be a more convincing demonstration of the continuing vitality of the constitutional principles upon which it is said to depend.

At the same time, important components of pragmatism have, in recent years, been recognized as being far more problematic than previously thought. This is certainly the case with respect to its faith in science and the expectation of social progress. Given the depth and character of the contemporary crisis of political principle (including the crisis surrounding the individualism which is central to both natural rights and pragmatism), the juxtaposition of natural rights and pragmatism, as here presented, remains peculiarly beside the point.

There is one last major difficulty with this book. Far too little attention is given to the problems arising from the legal component of constitutional adjudication. Jacobsohn treats the Constitution perdominantly in political terms, as the embodiment of political principle. While the Constitution certainly does embody political principle, the legitimacy of judicial review is anchored in the Constitution's status as supreme law. Any discussion of judicial statesmanship must proceed through a discussion of law. This is, perhaps, the primary lesson to be learned from the greatest judicial statesman in American history, Chief Justice John Marshall. Judicial statesmanship is the high point of the political component of judicial review. When successful, it can be, as Jacobsohn rightly argues, a source of critical political leadership. But it is better understood as the delicate and highly problematic culmination of the judicial craft, and not, as this book too often suggests, as its daily preoccupation.

SYLVIA SNOWISS

California State University, Northridge

Affairs of State: Public Life in Late Nineteenth Century America. By Morton Keller. (Cambridge, Mass.: Belknap Press, Harvard University Press, 1977. Pp. vii + 631. $17.50.)

Students of American political development will find Morton Keller's *Affairs of State* an important work. Keller has produced a major history of the late nineteenth-century "polity" through an examination of its public institutions. Keller constructs a portrait of American political life in this era by analyzing "party politics and elections, political thought and public issues, legal doctrines and court decisions, the work of legislatures and public officials" (p. vii). This analysis suggests a redefinition of the distinct political universe which appeared in the United States between the end of the Civil War and the Progressive Era.

This book merits a prominent place in what is emerging as a general reevaluation of late nineteenth-century America. Previously the post-Civil War era was regarded as the darkest age of American politics, a period in which political corruption was rife and the sole *raison d'être* of the government was to facilitate the unencumbered reign of new industrial capital over the nation. Contemporary scholars are turning away from these themes toward a richer description of the confrontations between modernity and tradition which occurred during these watershed years of American history. In this revisionist vein, *Affairs of State* stands as the most comprehensive challenge yet to the older view of late nineteenth-century American political life.

Along with others, Keller seeks to replace the simplistic image which tends to dominate treatments of public policy during this era. With his wide range of concerns and his mastery of historical materials, he demonstrates that the doctrine of laissez-faire did not go unchallenged at this time; the status of government was constantly at issue and continually changing in the volatile politics of these years. But Keller has not only added the force of scope and detail to current efforts in reinterpretation, but he has also attempted a distinct characterization of the period. His history focuses attention on the complex dynamics of change in public life produced by disjointed developments in the political, social and economic spheres. He continually draws attention to the boundaries within which major transformations in any one sphere were contained by the limited flexibility in others. With this perspective, Keller speaks for a cautious reappraisal of change within the period. Indeed, he directly criticizes some contemporary historians by noting that "preindustrial values had a longer, continually reinforced existence than is allowed by those who dwell on the transforming effects of industrialism" (p. 286).

The tension-ridden push and pull of political, social and economic forces provides the backdrop for a work which focuses on the ambiguities and uncertainties in the political developments of the time. Keller recognizes that this motif is hardly unique to the late nineteenth century in America, but he appeals to the striking character of the evidence to justify its particular relevance to an understanding of this era.

Two "historical generations" can be distinguished in the late nineteenth century, Keller argues, according to the developmental tensions which predominated in the periods 1865–1880 and 1880–1900. In the first, "the post-war polity," he describes the fate of a powerful wartime state whose rapid but superficial development had far outpaced the needs and orientations of American society. The governmental demands of the Civil War had built a modern state without firm roots in national life. Between 1865 and 1880, Americans turned their backs on strong central government, aggressiveness in national policy making, and the ideals of equal citizenship, and they resurrected their older norms of localism, laissez-faire, and racism. But this postwar reaction against the transformation of government was halted by the irrepressible social and economic effects of industrialism. During the second generation, "the industrial polity," new tensions emerged as the forces unleashed by economic revolution met older social and governmental norms. Uncertainties regarding public purposes and appropriate political means grew more intense after 1880. Courts, national parties, and state legislatures were stretched to the limits of their capacities in the attempt to meet the demands for an industrial state. The period closes with firm relationships between state and society yet to be established.

Keller details the vigor and creativity of the polity within these broad themes of the unresolved tensions produced by disjointed developments among the various spheres of public life. The weight of his interpretation rests on the breadth of his examination of public activity. The result is a pointed history in which argument illuminates, but never overwhelms, the richness and diversity of the subject matter.

Still, the reader finishes the volume recognizing the limitations of these virtues. While convinced that the late nineteenth century was not simply a dark age of irresponsible politicians and government lethargy, while clearly perceiving the inadequacy of equating the emergence of modern America with the presidency of Theodore Roosevelt, one is left only with the ambiguities of a vigorous polity searching to resolve ultimately unresolved tensions. Without a more developed statement of the distinct accomplishments and/or failures of the American political system during this period, the sense of definition tends to get lost. In the end, Keller's mastery of scope and detail and his keen appreciation of the ambivalence manifested in the late nineteenth century cannot serve as a satisfying substitute for the meaning of the age.

Affairs of State provides a firm foundation for much work still to be done. If Keller has not fully answered the questions of the distinctiveness and significance of late nineteenth-century America, he has provided the comprehensive view and general perspective which should inform any further study of political developments in this period.

STEPHEN SKOWRONEK

Cornell University

Watergate and the Constitution. By Philip B. Kurland. (Chicago: University of Chicago Press, 1978. Pp. x + 261. $12.50.)

There are several indications—enough to satisfy at least this long-time Court appointments watcher—that Philip Kurland was in the running for the vacancy on the highest bench in the land created by Justice Douglas' retirement late in 1974. Kurland would have adorned the tribunal—indeed he has long deserved to be there—but we would have been the poorer without his busy, trenchant, expert, sophisticated, candid commentaries on the state of government and politics in our land. *Watergate and the Constitution* is his latest contribution. Despite its admittedly essayist design—its close to a dozen brilliant but loosely connected essays run the gamut from "The Congressional Power of Inquiry" to "The Plebiscitary Presidenty"—is one of his best to date. It blends history, legal analysis, constitutional verities, and prescriptions, and is conceived to equip those who care and dare with the apposite philosophical and pragmatic underpinnings to embrace those principles of government that will achieve the requisite limits on power and authority so as to ascertain the republic's future.

Alas, although Kurland concludes his book on a hopeful note, it is not convincing. More convincing is his finding that the "flow to the government of power over the lives of Americans [which] has been at flood tide for generations ... [shows] still no sign of ebbing." The wounds and legacy of Watergate are still with us. Notwithstanding a host of suggested (and legislatively considered) reforms, we have enacted just one: public financing of some elections and it was quickly rendered partly nugatory, however justifiably, by judicial veto. No steps whatsoever have been taken, however, in what Kurland properly identifies as the two cardinal "distortions of constitutional government revealed by Watergate," namely: (1) Congress' "failure to perform adequately its function as a check on the executive," and (2) the "inordinate concentration of power in the hands of the White House staff." The second may well be the more serious. For the Carter campaign and Carter administration's assurances to the contrary notwithstanding, that staff continues to be huge, commands too-generous salaries, is generally devoid of genuine experts experienced in the governmental process, did not have to face the check of senatorial confirmation, and is essentially still responsible to no one save the president himself. Hence, Kurland, adapting an old-time Chicago politician's comments about his city's machine, tersely concludes that "the United States, like Chicago, 'ain't ready for reform yet.'" Why not? And why have we not learned more from the Watergate affair?

Essentially, Kurland suggests, because the body politic and its governmental machinery have been both unable and unwilling to harness the continuingly broadening centripetal forces of the presidency. This failure, Kurland holds, is in overriding measure due to the following facts of institutional *cum* political life: (1) the utter hopelessness of ever transforming the leak-prone Congress into an at once effective and responsible legislature and a meaningful adversary vis-à-vis the executive branch; (2) the essentially uncontrolled and uncontrollable executive bureaucracy which is a law unto itself (e.g., witness its contemporary club-swinging and *soi-disant* lawmaking in the Departments of Labor and Health, Education, and Welfare), augmented by the not sufficiently perceived "semi-permanent bureaucracy" on Capitol Hill; and (3) the continued manifestation of judicial activism, of judicial lawmaking, which Kurland terms "license" and which, like his distinguished colleague and friend, the late and sorely missed Alexander Bickel, he in effect blames in considerable measure for the governmental climate in which "Watergate" was conceived and functioned. The latter is a rather harshly facile indictment, and constitutes an exaggeration as well as an oversimplification—but it *is* an assuredly demonstrable fact that "government by judiciary" (to lower-case Raoul Berger's title) is at least a partial reality.

Concluding that the "ancient concept of separation of powers and checks and balances has been reduced to a slogan," Kurland thus warns that because of "Supreme Court license and congressional irresponsibility . . . the nation is now governed essentially, not by laws enacted by Congress, but by rules and regulations promulgated by the executive branch and by independent agency actions, purporting to be in compliance with the congressional will." If this be a conclusion at once sweeping and alarming, Philip Kurland's superb analysis points to its essential truth. We ignore it at our own peril.

HENRY J. ABRAHAM

University of Virginia

The Courts, Social Science, and School Desegregation. Edited by Betsy Levin and Willis D. Hawley. (New Brunswick, N.J.: Transaction Books, 1977. Pp. 432. $24.95.)

The 15 original essays included in this volume are derived from papers presented at an August 1974 conference of lawyers, judges and social scientists; they originally appeared in *Law and Contemporary Problems.* The theme of the conference was the role of social science research in the development of the body of law dealing with equal educational opportunities. The articles deal with six topics.

The book begins with essays which review the development of case law since *Brown* v. *Board of Education.* Frank Read confines his attention to school desegregation cases and concludes with a look at the 1974 Detroit cross-district decision. Betsy Levin and Philip Moise cover much of the same material but go into greater detail and also go beyond Read to discuss desegregation law as it relates to Hispano-Americans.

The consequences of school desegregation is a second topic. Four essays present the major research findings on the relationships between desegregation and achievement (Meyer Weinberg), race relations (Elizabeth Cohen), life chances (Henry Levin), and personality (Edgar Epps). These useful reviews are careful compilations of findings which indicate beneficial consequences of desegregation as well as research which concludes that desegregation has no effect or is actually deleterious. While to some extent they cover material included in Nancy St. John's *School Desegregation: Outcomes for Children* (New York: Wiley, 1975), they make a contribution on their own. For example, Weinberg reviews a large number of unpublished dissertations.

Some of the material included deals with issues which have received relatively little attention from either lawyers or social scientists. Thus a third topic considered is the relative *costs* of efforts to achieve greater racial balance among schools. Owen Fiss sets forth items which should be weighed by judges confronted by plaintiffs who seek desegregation plans which would require extensive busing. Derrick Bell uses a hypothetical school district to explore alternatives to mandatory racial balance which seemed to be the thrust of court orders a few years ago. The search for alternatives is particularly appropriate in districts which continue to have high levels of racial isolation because of the geographical distribution of the races. With extensive busing increasingly being met by white opposition, declining black support, and congressionally erected obstacles, racial balance is unlikely to be achieved in many urban districts, so Bell's emphasis on techniques for achieving equal educational opportunities in minority schools is well placed.

A fourth topic is the emerging problem of unequal treatment of minorities in schools which have desegregated. This phenomenon, sometimes called second-generation discrimination, is explored by Mark Yudof as well as by Levin and Moise. Yudof provides the most extensive treatment to date of the legal ramifications of suspensions and expulsions of black pupils. Levin and Moise provide briefer glimpses at ability grouping and discipline.

Gray Orfield's paper introduces a fifth topic, making desegregated schools actually integrated environments. In suggesting school changes which may facilitate desegregation, Orfield draws upon the experiences which he gained in several school districts while working with the Commission on Civil Rights. He suggests eight ways of contributing to successful desegregation.

Reynolds Farley provides yet another perspective by introducing some original demographic research on the linkage between neighborhood racial composition of selected central cities, their suburbs, and the extent of elementary school segregation.

Throughout this volume the authors consider the utility of social science research for the courts. Their conclusions are not very positive. Various suggestions (see particularly Cohen and Orfield) are made for getting social science to address the questions on which court decisions are likely to hinge.

This book should interest readers concerned with judicial policy making, civil rights, and policy impact; however, readers will be disappointed by the contributions of the three judges whose brief papers are included.

CHARLES S. BULLOCK, III

University of Georgia

The Forgotten Governments: County Commissioners as Policy Makers. By Vincent L. Marando and Robert D. Thomas. (Gainesville: University Presses of Florida, 1977. Pp. xi + 154. $8.50.)

During the 20 or so years since Lawrence Herson told us that we had "lost" municipal governments, extensive and often illuminating research has been conducted in the general field of urban politics. Despite this voluminous body of published work, we are now told by Marando and Thomas that we have somehow "forgotten" counties as a jurisdiction of local government. The authors are clearly right in stating that the amount of research on counties pales by comparison with that completed on cities. This state of affairs is not exactly enough to make us mount the barricades, but the authors do a very effective job of convincing us that county governments deserve a greater share of our professional attention than heretofore.

After a foreword by Thomas P. Murphy in which he summarizes the major themes addressed in the book, the authors introduce the reader to their approach. "Principally from the perspective of the county commissioner, we describe and analyze the counties' responses to public problems" (p. 8). This book is based on data generated from the responses of 253 county commissioners in Florida and Georgia. There is a helpful chapter in which we are given a short introduction to the historical role of counties, their relations with states, and some currently popular structural forms of county government.

The main portion of the book is devoted to an examination of four themes: (1) the frequency and severity of county issues; (2) the scope of responsibilities of county governments; (3) county commissioners' policy orientations; and (4) the objectives of county commissioners. The authors rely almost exclusively upon the responses from their sample to draw conclusions in each of these four areas.

For instance, when asked to indicate what they perceive to be the proper scope of their responsibilities, the county commissioners presented an argument for a complicated web of intergovernmental responsibilities that changed by issue area, by the extent of urbanization in their county and by state. These data, as the authors indicate, do not augur well for any attempt to impose uniformity of policy implementation either within states or on a state-by-state basis. The data further revealed a widespread attitude that counties ought to expand their services and wherever "necessary" provide the full range of services that are normally associated with cities. As the formerly rural areas continue to suburbanize, these commissioners are saying that rather than see the formation of new cities they would much rather have their own county governments adapt and expand to meet the new influx of population. Not many governments can resist the temptation to expand the empire; apparently, counties are no exception.

A few areas of criticism should be raised. Both the title of the book and the way in which the authors treat the subject create the impression that the data may be generalized to all United States counties. This is highly questionable. For that matter, the authors do not make a strong case for the representatives of their sample even within the confines of Florida and Georgia. Nothing of the flavor or excitement of politics comes through in this book. Frankly, the book makes county government sound very dry, when in fact there is probably a continuous stream of political battles that in intensity and meaning for the participants rivals any of those occurring in the national political arena. Finally, relying upon the aggregated responses of 253 county commissioners should not allow the authors to make statements about what any particular county government thinks or will do, yet they clearly make these inferences. To be entirely relevant to actual policy we would need to know the attitudes of a majority of commissioners from each county. We are not told anything about the distribution of the respondents. It is at least possible that the respondents are all in a minority position on their respective commissions on some or all issues.

Policy making in county government is an important and neglected subject. For calling our attention to it the authors deserve our thanks. The data they have collected are very informative. As the authors themselves admit, the book should be viewed as the beginning of much-needed research on county government.

ALAN J. WYNER

University of California, Santa Barbara

Working-Class Women and Grass-Roots Politics. By Kathleen McCourt. (Bloomington and London: Indiana University Press, 1977. Pp. 256. $10.00.)

From the research of Mirra Komarovsky and Lee Rainwater in the 1950s, white working-class women have generally been portrayed in the literature as housebound and politically uninvolved. Kathleen McCourt's study, based on a series of in-depth interviews of white working-class women, challenges this assumption.

McCourt's analysis takes the form of a comparison of the attitudes of working-class women involved in politically assertive organizatons with those of their inactive female neighbors. The neighborhood from which the sample was drawn is a white, Catholic enclave surrounded by black ghettos on the southwest side of Chicago. It was selected because it includes several recent community organizations active on issues related to housing, schools, and race. McCourt's sample is highly selective: 23 women were chosen for their political activism, 17 for their lack of involvement.

The women included in the sample have few background differences. All are white, all have children, and all but three live with husbands. About half graduated from high school, and one from college. Only one-fifth of the women work outside the home, and half of these work part-time.

No single factor was found to determine why some women chose political activism. McCourt hypothesized that political activism would be linked to nontraditional attitudes on marriage, child rearing, and the rights of women. In fact, support for nontraditional attitudes was more commonly voiced by the politically inactive women. Interestingly, men's resistance to their wives' working remained strong in the community: half of the husbands involved were characterized by their wives as completely opposed to their working, and only two percent as actively encouraging to working wives. A larger percentage of the politically active women favored increased occupational and educational opportunities for women.

McCourt further expected that the activist women would be more integrated and attached to the neighborhoods than their inactive counterparts. In fact, with attachment measured by length of residence and proximity to relatives, no difference was discovered between the two groups.

What emerges from McCourt's study is a more complex motivational process: a number of factors at different times influenced women toward political involvement. Membership in an organization sometimes was described as stemming directly from the recognition of a particular community problem, sometimes simply from an invitation to join. In general, however, the activists did rank higher on the scale measuring the sense of personal political efficacy, tending also to hold the conviction that neighborhood problems could be resolved through collective action.

McCourt found little to suggest that the political involvement of these women constituted a racial backlash. The women studied shared a fairly high degree of verbal commitment to the ideal of racial integration. This conviction, however, tended to be qualified by objections to remedies such as forced integration, and numerical dominance of black citizens in previously white neighborhoods. However, blame for community problems was fixed not on black families, but on institutions—school boards, real estate firms, local lending companies, and federal housing agencies.

McCourt's research is important for two reasons. Despite the massive research done since 1960 on political behavior, there has only recently been empirical research dealing with the relationships between politics, gender, and social class. Further, recent changes in the status and role of women have been most visible as middle and upper-class phenomena; the benefits of the women's movement—Title VII of the 1964 Civil Rights Act, the Equal Pay Act of 1963, and Affirmative Action programs have largely accrued to middle- and upper-class women. McCourt suggests that the dramatic changes of the past decade have also affected the politicization of working-class women.

The women studied by Komarovsky and Rainwater did not join organizations. A generation later, the women interviewed by McCourt have become politically involved, under certain conditions. Without comparative data it is not clear whether these southwest Chicago women are atypical, representing possibly a form of midwestern populism, or other idiosyncratic behavior. It is to be hoped that McCourt will go beyond the limitations of this dissertation and continue her exploratory work on the sociology of working-class women.

MARJORIE LANSING

Eastern Michigan University

Modeling the Criminal Justice System. Edited by Stuart S. Nagel. (Beverly Hills, Calif.: Sage Publications, 1977. Pp. 319. $17.50, cloth; $7.50, paper.)

This is volume 7 of the Criminal Justice System Annuals that are regularly issued by Sage Publications. Its stated purpose is "to discuss the relevance of deductive modeling to obtain a better understanding of (1) why the criminal justice system operates the way it does and (2) how it can be made to operate more effectively." The volume is organized in four parts: (1) general models of the criminal justice system, (2) modeling law enforcement and police operations, (3) modeling the judicial process, and (4) modeling criminal corrections and deterrence. Each chapter reviews the literature related to a specific topic. Thus, a major value of the volume for readers is the series of bibliographies at the end of each of the chapters. Most of the chapters also present some form of a deductive model and assess the empirical warrantability of that model with available data. Consequently, the book is also of value to those who wish to see in one place a number of different deductive models that have been subjected to empirical testing.

Although it is impossible to discuss each of 18 somewhat disparate chapters, several of the articles are particularly noteworthy. Robert C. Reich's "Can Justice Be Optimized?" raises some important questions for those who approach modeling from an overly naive and optimistic view. Reich points out that a model is a powerful system because it denotes managerial efficiency as well as enabling a user directly to experience a feeling of control. Models reduce reality to simple patterns of cause and effect. Outputs measured as police response time, court delay, and recidivism seem to be reduced to fungible benefits or costs that are easily compared to input costs. Problems appear to be rendered comprehensible, offering therapeutic help to those facing ambiguous and diffuse problem situations. However, fundamental social choices may be hidden in what appear to be simple optimizing problems. The key question is, what is to be optimized? This involves "the very frame of reference through which such 'costs' are recognized and evaluated. An allocation of police resources that minimizes the monetary cost of crime, for example, with the consequence that police response time will vary depending upon neighborhood wealth, may fundamentally alter the community's understanding of the meaning of crime and its assumptions about the role of the police in controlling it." When models are implemented without consideration of the really fundamental questions underlying them, models impose their own normative structures.

Reich raises questions about what happens to concepts that have principled meanings (e.g., the concept of a fair trial) when they become a part of an optimizing formula. "Viewed instrumentally, such constitutional principles are reduced to mere policies, to be qualified, partially applied or compromised according to the situation in which they arise. When defined by their uses rather than by their historical meanings, such values no longer possess an absolute significance, and their absence is not viewed absolutely either. Injustice is simply a miscalculation of costs that may be inconsequential if the fact-finding process nevertheless is deemed reliable" (p. 62). Many would disagree with Reich, but the questions he raises are extremely important and should be given serious consideration.

Werner Z. Hirsch, in "Production, Cost and Expenditure Determinant Functions of Police Services," provides a major literature review of a rather extensive number of studies, but also brings a critical eye to several previously well-accepted empirical studies. One only wishes that his comments were not buried so deeply in this volume. Any serious policy analyst who has relied on early studies of the effects of scale on output should read Hirsch's review. He points out, for example, that the well-accepted article by Schmandt and Stephens contains logically invalid inferences, as well as conclusions, that are based on statistically insignificant partial correlations. A number of other studies receive equally devastating examination.

This volume is a collection of chapters written by individuals and suffers from the problems of many such volumes. Authors in one chapter directly contradict the arguments of another. The quality is uneven. Some chapters seem closer to the goal set by the editor than others. However, overall this collection has more cohesiveness than many such collections and will be of considerable interest and value to many political scientists interested in public policy analysis in general, as well as that related to the criminal justice system.

ELINOR OSTROM

Indiana University

Gosudarstvennyi stroi SShA (The State System of the USA). Edited by Aleksandr S. Nikiforov. (Moscow: Iuridicheskaia literatura, 1976. Pp. 326. Rubles 1.48.)

Soviet analyses of U.S. national constitutional institutions and their analogues at the state and local levels are rare. This is only the fifth study in the genre—the first appeared in 1930, the most recent in 1958. It is the most sensitive treatment of the lot because, although it has its share of Soviet Marxist conventional wisdom, it contains less ideological simplification, is not as legalistically schematic as the others, and takes into greater account real political dynamics and complexities in its descriptions of how the institutions function and interact.

The book makes a major contribution to the Soviet literature in its treatment of the principles of the separation of powers and checks and balances and of their application in recent political practice. The authors attack the decades-old orthodoxy that presidential power must inevitably expand at the expense of Congress and that this is a reflection in the political realm of the growing concentration of economic power in the nation. They reach the conclusion, theoretically important for Soviet analysts, that, "the concentration of economic power is not accompanied by a directly proportional concentration of political power. . ." (p. 194).

A series of proofs and reasons are adduced, including ideological ones (the ruling class prefers a dynamic, oscillating stability in the relative power of president and Congress rather than a lengthy hegemony of either) and some practical political ones (Watergate, the autonomous power of committee chairpersons, the recent reassertion of Congress' oversight function, and the democratization of political institutions and processes following the 1960s). Congress' feedback function is noted several times, and the assertion is made that whereas in the past the public opinion factor could be ignored, by the 1970s it had become a "formidable force" (p. 248). The function and factor are new in this genre of writing, and they are only rarely mentioned in any Soviet treatments of American politics despite their obvious importance, which is, moreover, hardly recent.

The same can be said about some innovations in the data base, and these are a significant sign of growing sophistication among Soviet analysts. *The Congressional Quarterly Weekly Report* is very extensively utilized in addition to the scholarly books and the popular periodical press normally used by Soviet writers in the past. For the first time ever in this genre political science journals are cited (the *American Political Science Review* and the *Midwest Journal of Political Science*) as are several papers presented at the 1973 APSA annual meeting. Use of these sources in the general Soviet literature on American politics literally can be counted on the fingers of one hand. Moreover, political scientists are here at long last identified as such rather than as sociologists, which has been the usual practice of Soviet writers.

In this volume American politics become more recognizable to the American reader than in previous Soviet studies of our institutions. The treatment of the court system in this updated description-analysis is, for example, the clearest, most insightful and understanding yet produced. Owing to the infrequent publication of books in this genre, each must cover an enormous territory—the mere updating of developments since 1958 could alone comprise several volumes. The authors deserve credit for moving their consideration of American politics as far ahead as is possible under Soviet conditions.

RICHARD M. MILLS

Fordham University

The Delivery of Urban Services: Outcomes of Change. Edited by Elinor Ostrom. (Beverly Hills, Calif.: Sage Publications, 1976. Pp. 320. $17.50, cloth; $7.50, paper.)

This volume represents the first of two 1976 editions of the prestigious *Urban Affairs Annual Reviews* series. (They might now be more appropriately titled *Urban Affairs Semiannual Reviews.*) Elinor Ostrom has brought together 11 original articles, unified, she says, by two themes. First, she suggests, "institutional arrangements *do* make a difference." Second, the "collective wisdom" of the collaborators favors diversity, so that "the wide range of institutional arrangements found in America's metropolitan areas is seen as an asset" (p. 8). To her authors she addressed a common question: "What do social scientists know that can be useful to public officials and citizens in organizing the delivery of urban services more effectively?" (p. 7) The answer appears to be: "not very much but we are trying harder."

One might think that a volume with this title would immediately address the ambiguous concept of service delivery. My intuitive notion suggests that delivery involves the transference of some valued commodity from a producer-supplier through some institutional mechanism or market to an ultimate consumer or citizen. Yet the conception of urban service delivery implicitly incorporated here (there is no explicit discussion of it) is apparently boundless. Zauderer's paper describes the electoral impacts of a 1949 Ohio ballot-form change, and has about as much connection to urban service delivery as, say, the work of the Survey Research Center. Luverne Cunningham's rather descriptive piece on Detroit school groups tells us about the value of an Ohio State seminar on "Detroit Decisions." Smith and Rose analyze public opinion on water policy in Washington State, but seem unaware of Ostrom's query to her contributors. An excellent essay by Robert Bish disputes the premises of *Serrano-Rodriquez* school finance litigation, but its connection to service delivery is no stronger than the assumption that fiscal variations produce service variations. A clearly articulated conceptualization of service delivery would have either raised questions about the logic of these inclusions or spurred a much more elaborate editor's introduction.

Fortunately, better than half of these chapters have something to do with urban service delivery. Most valuable are four articles representing preliminary reports from the NSF-RANN projects on the "Organization of Service Delivery in Metropolitan Areas." Here serious attention is paid to Ostrom's questions about institutional arrangements, diversity, and effectiveness, insofar as the latter idea can be tackled empirically. E. S. Savas, who writes on solid waste collection (a service which is sort of a "delivery in reverse") has elsewhere written forcefully about the pernicious effect of bureaucratic monopoly. His article here merely describes the many combinations of delivery systems in solid waste, and stops short of addressing his own question about "who can best perform" these services (p. 227). Philip Coulter and colleagues do address directly the question of what institutional arrangements for fire protection services are most effective, coming closer than any of the other authors to taking their editor's agenda seriously. Next closest is Roger Parks, writing about police patrol in metropolitan areas. Though evidence has recently piled up indicating that police patrol may not be very "effective," Parks settles for what can be reasonably measured, i.e., level of patrol deployment. Contrary to reformist wisdom, he shows that fragmentation of metropolitan policing gets more officers on the street. Finally, Patrick Donoghue examines the effect of various market forces on the distribution of physicians.

Certainly these articles support Ostrom's contention that institutional forms matter. Yet that fact, as she acknowledges, is not too much disputed. More uncertain is whether the "collective wisdom" of these articles demonstrates that diversity in institutional arrangements is an asset. Probably the most "diverse" service examined in the volume is federal housing policy, which Roger Allbrandt carefully reviews and proclaims "not . . . a resounding success" (p. 15). Supporters of diversity will be encouraged by Parks' finding that fragmented police systems get more personnel on patrol. But it is difficult to take that next step and link patrol officers to service effectiveness. If these articles may sometimes raise more questions than they answer, they do point up the significance of Ostrom's question about service effectiveness.

Frankly, I would look forward to a volume 20 of the Sage series which once again asked what social scientists could tell urban governments about more effective service delivery. The field of public administration, long dominated by research on organizational contributions and organizational behavior, has sadly neglected organizational services, particularly at the urban level. It is this gap—a chasm, really—which the present volume tries to fill. To the degree that this rather eclectic collection in volume 10 of UAAR has made a contribution (and several of the papers do), volume 20 will be an easier one to edit.

ROBERT L. LINEBERRY

Northwestern University

Leadership in Congress: Stability, Succession, and Change. By Robert L. Peabody. (Boston: Little, Brown, 1976. Pp. xx + 522. $13.95, cloth; $7.95, paper.)

Analysts of the U.S. Congress have made good use of case studies. These studies bring life to the barren outlines of the formal law-making process and acquaint readers with legislative personalities whose formidable skills of manipulating the internal power levers are too often overlooked by the superficiality of the national media's coverage of Congress.

As beneficial as they may be, case studies have their limitations. Because of the partial quality of the analysis and the atypicality of the cases chosen for examination, they are often regarded as unrepresentative "snapshots" of a process and not full-bodied depictions. And as we are often reminded, theory-building aspirations are not satisfied through case analysis.

Robert Peabody's book, *Leadership in Congress: Stability, Succession, and Change* is a book which combines case studies with an important theoretical framework. It deals with every congressional floor leadership change from 1955 through 1975. Thus, it satisfies the demand of completeness while at the same time providing ample illustrations for the testing of his framework.

Turning first to the cases themselves, the Peabody style is distinctive. Each case selected for full examination opens with a sensitive analysis of the relative power positions of each combatant; the combatants' place within the ideological structure of his or her party; the personality characteristics which can enhance or endanger the voting coalitions behind each candidate; and the shifting alliances which occur throughout these intraparty contests. Each of these assessments is well written and enlivened by insightful quotations from various members, observers as well as participants.

Of the recent academic analysts of Congress, no one writes better "insider" commentary about life on Capitol Hill than does Robert Peabody, who occupies a special place in the congressional research community. His book is the fullest collection of studies in this mode. It includes eight detailed case studies: in addition to four in the House, there are studies of the Albert-Bolling parry of 1962 (written by Nelson Polsby); the Ford-Halleck contest of 1964–65; the Boggs-Udall-Sisk-Hays-O'Hara free-for-all in 1971; and the abortive Gibbons challenge to O'Neill in 1973. Four cases come from the Senate, three involving the Democratic whip post (Long's victory in 1965, Kennedy's in 1969, and Robert Byrd's in 1971), the fourth concerning the Scott-Baker minority leadership fight of 1971. In addition to these, Peabody makes reference to many of the other battles which occurred during this period of congressional history.

As valuable as the case histories are, the book's benefit is further increased by Peabody's excellent chapters on the nature of party leadership within each chamber. They are very useful introductions to the contemporary structure of power within each congressional body.

I enjoyed the book. But there are problems with the selection of case material and the reliance upon the leadership change framework which Peabody has been utilizing for more than a decade. None of the examined Democratic contests in either chamber involved the top leadership post (i.e., the House Speaker or the Senate Majority Leader). There is much to be learned from a detailed analysis of the McCormack-Udall vote of 1969 in which a sitting speaker's nomination was contested in his party's caucus for the first time since 1923. It was the first time a sitting Democratic speaker had faced caucus opposition since 1879. Udall's willingness to confront the remnants of the Rayburn-McCormack dynasty which had held sway over the Democrats since 1940 was much more important than Sam Gibbons' decision not to contest Thomas O'Neill for majority leader in 1973. Yet, the Gibbons' non-contest received over 30 pages in the book while the McCormack-Udall contest which resulted in actual votes cast in the caucus received little more than two paragraphs.

Also, Peabody's selection of the period, 1955–1975, leads to the inescapable blurring of party identity and party status. All ten Congresses in this era were controlled in both chambers by the Democratic party. This means that assertions about minority party behavior pertain only to the Republicans.

The contentious behavior of the House Republicans who overthrew three leaders may be a function of that party's unique political history and not simply a response to the frustrations of life as a congressional minority party. This same status-identity blur is even more pronounced for the Democrats. Not only were they the sole majority party throughout this era but also for 12 of the 20 years incuded here, they were a "truncated majority" obliged to function with a president of the opposing party. As Randall Ripley notes in his book, *Majority Party Leadership in Congress,* the role of the majority leaders in these truncated circumstances is quite different from that exercised during periods of presidential-congressional party agreement.

Other reservations which I have about the book relate to Peabody's leadership change framework and his assumptions about "succession patterns" and "heirs apparent." In both the House and the Senate, it is clear that the Democrats move their leaders in an upwardly vertical pattern from whip to floor leader (and to speaker). But neither the House nor the Senate Republicans appear to do this consistently. Senator Robert Griffin's thwarted bid in the 95th Congress to move from whip to leader is only the most recent instance among Senate Republicans of the absence of a "pattern of succession." Republican floor leaders have come from various locations within the party's leadership configuration. The whip post which has been historically prominent in the case of the Democrats has been a "dead end" for all Republican whips since 1905 in the House and for 7 of the 11 Senate Republican whips since 1915. Thus, Republican caucuses cannot be accused of violating patterns of succession which they do not possess. A fuller treatment of the differences between the congressional parties in their selection patterns would have led to clearer pictures of the distribution of power within each.

A final reservation about the "insider" perspective itself is that too close a proximity to an event may lead one to attribute characteristics to it that later analysis may dispute. For example, Peabody states that "contests" occurred in three instances in which no dissenting votes were cast in the caucus: Albert versus Bolling, 1962; O'Neill versus Gibbons, 1973; and Rhodes versus Arends, 1973. With regard to the last election, he contends that "John Rhodes' unanimous election as minority leader on December 7, 1973 represented . . . an example of open competition. . ." (p. 284).

The same problem occurs in Peabody's classification of John Conyers' two challenges to Carl Albert. In 1971, Majority Leader Albert defeated Conyers for the speakership nomination, 220 to 20. Two years later, Albert was renominated as speaker by the caucus over Conyers by a vote of 201 to 25. On page 293, both of these votes are treated as "contests," but on page 276, Albert's ascension to the speakership is called a "routine advancement," one in which "the next-ranking member in the party hierarchy is elevated without challenge." For Peabody, there is no ambiguity because his proximity to both votes indicated that Conyers' 1971 bid was a "symbolic" one but that his 1973 bid was a "real" one. For observers of Congress less close to the Capitol, the similarity of the vote and the results would seem to indicate that this was a distinction without a difference.

These classifications are obviously understandable to a person whose presence on the Hill at the time allowed him to conclude that some unanimous caucus decisions represented genuine consensus while other unanimous decisions represented an artificial one. In the same vein, this perspective also permits one to determine that voted opposition in the caucus may be "symbolic" sometimes and "genuine" at other times. However valid these distinctions may be to the observer, thay may be less helpful to the reader. A simple way to avoid this terminological blurring would have been to use the term "challenge" to refer to any instance of possible competition for a leadership post and to use the term "contest" to refer to the actual manifestation of votes in the caucus for more than one candidate. I believe that readers would have benefited from the consistent application of this distinction.

Putting all reservations aside, there is no question that this is an important book for congressional analysts. It brings together in one sizeable volume a vast wealth of information about the inner workings of the four congressional parties whose interactions with one another do much to shape the contemporary world in which we live.

GARRISON NELSON

University of Vermont

The Election of 1976: Reports and Interpretations. By Gerald M. Pomper with colleagues. (New York: Longman, 1977. Pp. viii + 184. $8.95, cloth; $3.95, paper.)

Hot on the heels of Jimmy Carter's triumph, Gerald Pomper and four of his Rutgers colleagues (with the assistance of the rest of the Pomper family) assembled this slender volume, intending to "report the results and probe the significance of the election of 1976" (p. v). At the very least, the book represents an act of raw courage, for in the interest of timeliness the authors have set themselves up to be second-guessed by the numerous studies of the election which have already begun to appear, based on better data than were available to them.

But *The Election of 1976* is a good deal more than that. It is a comprehensive, insightful yet concise portrait of an election year, and it holds up remarkably well more than a year later. Pomper himself offers two chapters, one on the nominating process and one on the presidential election proper; Henry A. Plotkin discusses in one chapter the campaign issues and in another, the state and local results; Charles E. Jacob writes perceptively about the congressional outcome; Ross K. Baker tries to forecast the Carter administration; and Wilson Carey McWilliams offers his all-too-brief speculations on the meaning of the election. The book is well written, attractively packaged, generally well coordinated (although Baker's chapter overlaps Jacob's and one of Plotkin's), and even has a few intramural disagreements for the reader to ponder. Perhaps most surprisingly, this "rush job" has remarkably few factual or stylistic lapses, although I was rather startled at Pomper's revelation that Gerald Ford had "literally" wrapped himself in the flag (p. 69)!

Many thought-provoking questions are presented. Was it a "critical election?" Pomper and McWilliams gingerly propose the possibility, and Pomper bases his claim in part on low correlations between Carter's vote and the votes for Johnson, Humphrey and McGovern. Had he instead correlated the Ford vote with earlier Republican votes, he would have discovered that except for 1964 and 1972, the correlations for most recent years are respectably high, and suggest no realignment. Baker, following James David Barber, presents Carter as an "active-positive" president. (Unfortunately, the Barber typology is not fully explained.) There are those who see the president as active-negative, and his chilly smile certainly not indicative of the sense of humor that is so important to Barber (does he ever laugh at himself?). McWilliams attributes Ford's strength, despite the recession, to our "contract society" (p. 153) and the primacy of moral and social issues. It can also be argued that the lack of sharp issue differentiation perceived by the electorate (*pace* Plotkin) led many voters to fall back on candidate image and regional and religious prejudices.

But these differences of opinion are only intended to demonstrate the grist for reflection this book offers. There are two general criticisms I would make, however. The first is the authors' frequent straining to make the election more interesting, significant and unique than it really was. It was, we are told, "exciting," a "vital time" (p. v), with "significant" disagreements between the candidates and the platforms (p. 43), perhaps ushering in a period of one-party rule (p. 161) or at least a realignment; and even a New Jersey referendum on gambling in Atlantic City is said to have "far-reaching" impact (p. 113). In a more sober mood, Pomper acknowledges at the end of his chapter on the nominating process that the features of that process in 1976 were a continuation of a long trend, and not unique to 1976 as earlier passages imply. As for the election itself, simple partisanship seems to have played a greater role than in any other election for at least 16 years. The only author who explicitly denies the earth-shaking importance of these events is Baker. The others seem to want the election to have been an 1896, while history may see it as perhaps a 1960.

The second general point is that the book could use a unifying theme. In other works, Pomper has shed light on how effective our elections are at linking popular preferences to governmental policy; why not apply this question to 1976? Or, what did that year tell us about our parties? Pomper and McWilliams advocate some form of party government—why not unify the book around that?

But these shortcomings do not seriously weaken the book, which will be useful in undergraduate courses. I hope they do it again in 1980.

HOWARD L. REITER

University of Connecticut

Handbook of Political Socialization: Theory and Research. Edited by Stanley Allen Renshon. (New York: Free Press, 1977. Pp. 547. $19.95.)

According to the editor, the purpose of the *Handbook of Political Socialization* is to (1) review the state of current knowledge in the field of political socialization; (2) assess methodological techniques and strategies of inquiry; (3) assess current and past research, and (4) suggest promising new lines of inquiry (p. vii). The essays in this volume are only moderately successful on the first point and deteriorate progressively as one moves through to point four.

There are a total of 15 essays in this collection. The chapters are broken into a theoretical/methodological introduction with articles by Renshon, Weissberg and Joslyn, and Nathan and Remy; a section on the role of "agents" with articles by Beck, Davies, Silbiger, Patrick, Chaffee et al., Sigel and Hoskin and Cutler; a section on "outcomes" with articles by Friedman, Simpson, Tolley and Gergan and Ullman, and a final chapter on "possibilities" by Harold Lasswell.

The book has a rather decided slant which reflects the editor's own interests in political socialization. In short, there is an emphasis on socialization from the perspective of micro-level psychology. The "political" aspect of political socialization receives less attention, I think, than most political scientists might prefer. For example, the "political theory of political socialization" developed by Easton and Dennis receives about ten pages of attention. Much of the work stimulated by the systems approach to political socialization is barely mentioned. There is no extended discussion of the concept of the "benevolent leader" and its implications. On the other hand, many social psychological theories are discussed at great length. Friedman in his article on "Political Socialization and Models of Moral Development" has a 25-page discussion (in a 32-page essay) on the theories of Piaget, Kohlberg and their followers. The few pages devoted to the political aspects of the moral development approach are exceedingly superficial. Many of the explicitly political works in the area are never mentioned. There is only the most cursory discussion of socialization to the law and to legal norms.

Unfortunately, the shortcomings of the Friedman article tend to be the rule rather than the exception. Two of the articles claim that so little is known about the subject to which they have been assigned to write that they must employ some tactic other than a conventional literature review. Chaffee et al. in their article on socialization and mass communication report on some original research. Sigel and Hoskin, in their essay on adult socialization, rehash some fairly well-known information on the effects of aging, occupation and status.

Many of the articles pay relatively little attention to political socialization as either a process or as a link to political structures or outputs. Often some dependent variable is chosen, much attention given to its correlates, and then a few pages are devoted to what are normally thought of as questions of political socialization. Examples of this approach are the articles by Elizabeth Simpson on "Values in Politics" and the article by Gergan and Ullman on socialization to political activism. In the Simpson article approximately 20 of 25 pages are spent on a discussion of non-socialization research on values, such as the Allport, Vernon and Lindzey *Study of Values* or the Rokeach study of instrumental and terminal values. In the Gergan and Ullman article on political activism 33 of 39 pages are expended recounting (à la Lester Milbrath's *Political Participation*) the correlates of political activism.

Although the editor claims to be inventorying the major trends in the field of political socialization, the volume contains much of what I, at least, consider to be rather peripheral. The best example is James C. Davies' article on "Political Socialization: From Womb to Childhood." Davies' "most basic proposition" is: "[a] child who has been severely physically deprived through the first six years of life will very likely not become involved in any political decision making as an adult" (p. 145). He concludes that "massive research" is needed on the question of "what correlation is there between the mental effects of malnutrition, loss of emotional support, and sudden powerlessness in early childhood and diminished ability in adulthood to make public choices" (p. 151). I find all this rather esoteric and not the sort of thing that should be included in a volume with the purposes and goals of the *Handbook of Political Socialization.*

Another distressing aspect of the book is the general failure of the authors to engage in any very incisive critical analysis or to suggest promising new lines of inquiry as was the charge of the volume's editor. The suggestions for additional research were usually (1) pitched at too high a level of generality to be of much use, (2) unrealistic given the current and projected future state of the discipline, or (3) mundane or trivial—i.e., the oft-repeated call for more longitudinal research.

On the positive side, however, there were several articles that I found to be useful and informative. Robert Weissberg's and Richard Joslyn's article on the methodological appropriateness of political socialization research makes some excellent points on the problems of studying socialization in the family and problems of comparisons in subgroup analysis. Also quite good is John Patrick's article on political education and political socialization. Patrick takes a straightforward approach to what is desired from civil education, what are the problems, what are the results, and what might be done for improvement. The article by Neal Cutler on the cohort and lineage approach to political socialization also merits attention. Concepts and methodologies are clarified in a useful way, and what can and cannot be learned from each approach is clearly spelled out. Finally, Renshon's own introductory essay is a useful overview of the assumptions and approaches to the field.

Students of politics who are not specialists in the subfield of socialization will probably find this book more useful than specialists. On the whole I find the book somewhat disappointing, although those who are more psychologically oriented than I may be more positive in their evaluations.

KENT L. TEDIN

University of Houston

Exclusionary Injustice: The Problem if Illegally Obtained Evidence. By Steven R. Schlesinger. (New York: Marcel Dekker, 1977. Pp. vii + 116. $13.75.)

Exclusionary Injustice is a relatively traditional treatment of the policy implications of the rulings of the U.S. Supreme Court on illegal searches and seizures. Schlesinger begins with an historical survey and critique of the Court's decisions regarding the exclusionary rule, accusing it of: (1) providing no protection/compensation to innocent individuals whose rights have been violated; (2) overprotecting guilty defendants; (3) providing little deterrence to illegal police behavior; (4) freeing guilty defendants (because the most credible kinds of evidence are excluded), thus endangering society; (5) undermining public respect for the court system; (6) failing to discriminate between more and less serious offenses and willful or flagrant constitutional violations; and (7) encouraging plea bargaining. While this is a reasonably comprehensive treatment, these criticisms have been frequently articulated by others, so little in this section of the book is original.

As a remedy to these social maladies, Schlesinger proposes that the exclusionary rule be eliminated and independent boards to review police conduct be substituted in its place. Composed of citizens, judges, law officers and others, the boards would be given powers to sanction individual officers and provide compensation to illegally abused citizens. Successful claims resulting from civil actions would be paid from the public treasury. Further, Schlesinger advocates statutory enactment of incentives to facilitate successful tort actions (e.g., plaintiffs would only have to prove that their rights had been violated; compensation would be a function of the degree of harm done). Illegally gathered evidence would then be admissible in court. Such a program, according to Schlesinger, would remedy the major deficiencies in extant policy governing illegal searches and seizures.

Serious inadequacies exist in both the author's treatment of the evils of the exclusionary rule and in his proposed remedy. First, his critique of the rule demonstrates little acquaintenace with basic social science findings relating to the criminal justice process. For example, the suggestion that police are unconcerned about cases after arrest, and are therefore not deterred by the exclusion of evidence at trial, totally ignores an already considerable, and growing, body of organizational research (e.g., Eisenstein and Jacob; Neubauer, etc.) which demonstrates powerful interagency constraints. By failing to appease prosecutors (who are vitally interested in trial proceedings) the police would not be able to obtain cooperation on prosecutions of great interest to them. Regrettably, this sort of inattention to the findings of the empirical literature is pervasive throughout the book.

Several flaws in the author's proposed alternative to the exclusionary rule can also be identified. His proposal rests on the assumption that individuals can and should be responsible for protecting themselves from abuse by the state. If the individuals likely to be abused by the police were a cross-section of society, this *might* be justifiable. However, different segments of society are not equally subject to abuse, so the author would place the onus on one of the least powerful segments of society to correct the abuses of one of the most powerful. Such a plan would probably not have a high rate of success, even if court-appointed attorneys, etc., were provided, and Schlesinger's selective review of our previous experiences with civil review boards does little to assuage these fears.

The debate over the putative evils of the exclusionary rule is frequently tinged with thinly veiled ideological assumptions. At this point, the debate can be clarified only through rigorous social science research. Whether Schlesinger's "countless" individuals are actually being freed by the exclusionary rule can be answered only by systematic empirical research. Perhaps a moratorium on books such as this ought to be declared until such evidence is assembled.

In sum, Schlesinger's book adds little new to our understanding of the exclusionary rule. The book is simply an extended version of Burger's dissent in *Bivens* v. *Six Unknown Named Agents*—little is gained from the book that cannot be found in the much shorter dissent.

JAMES L. GIBSON

University of Wisconsin, Milwaukee

The Public Use of Private Interest. By Charles L. Schultze. (Washington, D.C.: The Brookings Institution, 1977. Pp. vii + 93. $7.95, cloth; $2.95, paper.)

Charles L. Schultze, now chairman of the president's Council of Economic Advisors, was a Brookings senior fellow and professor of economics at the University of Maryland at the time the material for this book was prepared for presentation in the Godkin Lectures at Harvard University in 1976. Although the book is a revision of the earlier lecture material, redundancy between chapters and the lack of a table of contents inconvenience the reader. Overall, the book is worth reading, however, as Schultze addresses a problem inevitably affecting the governability of the U.S. political system: "When and how should government intervene in private markets for the purpose of improving economic efficiency?" (p. 1) According to Schultze, as the volume of regulatory decisions made by government grows, "the strain upon the political fabric grows at least proportionately" (p. 75). Yet the problem of social intervention at the micro level cannot be resolved within the framework of the traditional conservative-liberal debate of more or less government. This is because in the last 20 years the arrival of the Welfare State has changed the nature of the governability problem. Today as government intervenes in ever more complicated areas influencing the decisions of millions of individuals and business firms, "the critical choices have a much lower ideological and ethical content" (p. 89).

Schultze does not recommend abandonment of the political process or the separation of administration from politics, but instead emphasizes the present policy dilemma: "How does an ingrained political process, which stresses value adjustments, come to grips with the critical choices among technically complicated alternatives when some of the very political techniques that move society toward a decision themselves make it difficult to pursue workable methods of intervention?" (p. 89) Schultze sharply criticizes James Buchanan and his public choice colleagues who would deal with this dilemma by calling for a constitutional revision which would restrict the scope of allowable intervention. Schultze is adamant in his belief that "there is a growing need for collective influence over individual and business behavior that was once the domain of purely private decisions" (p. 5). The intervention technique called for by Schultze is to imitate the market mechanism by designing strategies which focus on self-interest incentives. Essentially this is a refinement of the argument set forth in an earlier work ("The Role of Incentives, Penalties, and Rewards in Attaining Effective Policy," in *The Analysis and Evaluation of Public Expenditures: The PPB System,* U.S. Congress, Joint Economic Committee, 91st Congress, 1st Session, 1969, pp. 201–25). As Schultze explains the new intervention strategy: "The boundaries of the "public administration" problem have leapt far beyond the question of how to effectively organize and run a public institution and now encompass the far more vexing question of how to change some aspect of the behavior of a whole society" (p. 12).

Schultze's case for behavior modification techniques as the answer to the "public administration" problem is grounded in a narrow view of political economy. He states that "we segregate our approaches to social organization into two watertight compartments—command-and-control techniques for public intervention and economic incentives for the private economy" (p. 67). He laments the fact that "we usually tend to see only one way of intervening—namely removing a set of decisions from the decentralized and incentive oriented private market and transferring them to the command-and-control techniques of government bureaucracy" (p. 6). This description of social organization in the U.S. is incomplete. The existence of command-and-control techniques in the private sector and incentive-oriented intervention strategies in the public sector are glossed over. Although Schultze is concerned with the public use of private interest, he does not discuss the problems of using a decentralized incentive-oriented intervention strategy to control large corporations. Also, the argument that government is biased against incentive systems is overstated.

The Public Use of Private Interest leads one to the conclusion that too much social regulation is responsible for the current strain on the political system. As Schultze contends, "The toughest challenge to the political consensus necessary to hold a free society together arises when a society itself has to make explicit decisions about the fate of particular groups and communities" (p. 75). Schultze would incorporate the market mechanism into the public sector as a type of safety valve which would dissipate tensions in the political system. One question that needs to be raised is whether too much governmental regulation is a symptom rather than a direct cause of the current policy-making dilemma. If the current crisis of governability is caused by too much democracy as the Trilateral Commission would suggest, or a fiscal crisis of the state as some radical critical theorists maintain, then the linkages between Schultze's new social intervention strategy and these other social forces must be more clearly drawn.

GREG J. PROTASEL

Oregon State University

Economic Regulation and the Public Interest: The Federal Trade Commission in Theory and Practice. By Alan Stone. (Ithaca, New York: Cornell University Press, 1977. Pp. 314. $15.00.)

As long ago as 1954 Harvard Law Professor Louis Jaffe was pointing out to political scientists that their "captive agency" theories of regulatory agencies were wrong because most of the regulatory agencies established before World War II did not have to be captured by industry—the statutes they administered were designed to serve industry goals of stabilizing and restricting competition rather than promoting competition to protect the consumer. In this study of the FTC, Alan Stone has provided political scientists with some of the best documentation yet of this important insight. To be sure, Stone disagrees with historian Gabriel Kolko and his followers who argue that the regulation of business can be explained solely in these terms. Stone finds that some of the statutes administered by the FTC such as the original Federal Trade Commission Act and Clayton Act serve the contradictory goals of promoting competition and restricting competition while others like the recent truth-in-lending and warranty reform laws restrict competition for the legitimate purpose of protecting consumers even though they have largely failed to do so.

Stone also takes issue with the Naderites who in their 1969 report on the FTC focus on the personal characteristics of the commissioners and staff and allege laziness, incompetence and a bias in favor of big business. To the contrary, the author notes that there have been times when Commission leadership was courageous and innovative including its mid-1960s decision to regulate cigarette advertising, the recent move to ignore enforcement of the anticompetitive Robinson Patman Act and early periods of tough antitrust enforcement against large and small firms alike. Stone acknowledges that much FTC enforcement is oriented toward small firms, is industry-initiated and mired in long delays. But he points out that these behavioral characteristics are the consequence of structural factors: the intense competitiveness of small, marginal firms in relatively atomistic markets, the relative lack of nonbusiness sources of information and the procedural requirements of the law. Since the author worked for eight years as an attorney with the FTC before entering graduate school, his comments on the relative importance of goals and personalities carry considerable weight. Despite its emphasis on legislative goals, however, *Economic Regulation and the Public Interest* turns out to have a second theme which is much more important to the book's conclusions and of greater significance to public policy debates on regulatory reform.

Put simply, Stone's larger argument is that the FTC's generally poor performance and limited impact, points on which many scholars of the left and the right agree, cannot be explained simply in terms of competitive and noncompetitive policies cancelling out each other or in terms of the vagueness of statutory language, and major point in the work of some scholars of regulation. The weakness of the FTC's traditional remedy, the cease-and-desist order, and its outmoded discovery techniques are also brushed aside as critical factors. Stone argues that the FTC has had little impact on business behavior even when it has implemented laws with narrowly defined and noncontradictory goals, such as the anticompetitive Wool Labeling Act of 1939, and when innovative remedies (injunctions, divestitures, fines, seizures) and adequate discovery techniques were present. Rather Stone finds a number of basic defects in the type of trade regulation practiced in the United States, defects which he implies also characterize other government regulatory programs.

These faults include (1) the limited time and monetary resources available to regulators, particularly given the millions of business transactions carried on each year; (2) a private litigation model of administrative procedure which inevitably leads to long delays and thus gives targets of enforcement multiple opportunities to continue violations of the law; (3) inadequate sanctions or tougher penalties that require strict burdens of proof by the Commission and thus are unlikely to prove effective (even the "fly-by-night" con man for whom the latter seem intended are not deterred, according to Stone, since they know that the odds of even getting caught are slim); (4) broad discretion available to regulated firms to find ways to defeat the purpose of the government's prohibitions, including psychological manipulation of consumers and transferring to the consumer the considerable costs of tougher regulations when they are strictly enforced.

Noting the recent upsurge in regulatory costs compared to what he considers to be regulation's meager benefits, Stone concludes that "on balance, the kind of business regulation we now have is not and cannot be an effective instrument for the promotion of public values" (p. 263). This may sound vaguely reminiscent of the Chicago School economists, but Stone is coming from the other end of the political spectrum. He believes that trade regulation is doomed to failure as long as producers are free to "psychologically manipulate" consumers through advertising appeals that distract them from "rational" consumer choice. Not surprisingly, this notion leads him away from capitalism altogether. Apparently Stone believes that if the marketplace cannot be perfected, one should get rid of the market. Thus he disposes of numerous regulatory reform measures: deregulation, a return to small-scale enterprise, an isolated antitrust attack on the super-giants, and public utility-type control. Stone then tentatively concludes that outright public ownership may offer the best alternative to present regulation since it is the only system in which the incompatibility of profit and sales goals and "public values" is resolved.

Having thrown the baby out with the bath water, the author tries to reassure the reader by suggesting that socialism may not lead to totalitarianism when practiced in countries with a democratic tradition. Even granted this latter point, the author's concluding discussion of regulatory reform alternatives is extremely superficial and dated. There is no mention of the liberalized or new market-oriented approaches to regulation (e.g., liability laws, pollution taxes), no set of distinctions employed to separate different regulatory problems and absolutely no discussion of the basic reasons why the relatively nonmarket economies have been so inefficient or why some of them have had such poor records in controlling externalities such as pollution. Nor does the author mention the essential truth of any economics—there is no free lunch under capitalism or socialism. Every dollar spent by a socialist society to protect the consumer or the environment is one dollar less available for some other social purpose.

His tentativeness notwithstanding, Stone should have avoided ending a narrow but good political science case study with an analysis of so broad and economically complex a subject as comparative regulatory systems unless he was prepared to discuss the subject with some sophistication. However, the case study itself does not suffer from such defects. Readers should simply be aware that much of the author's analysis depends on his assumption that the FTC has had little impact anywhere—an assumption that scholars of the left and right find congenial but which moderates may not like when they realize its implications.

PAUL J. HALPERN

Georgetown University

Judicial Impact and State Supreme Courts. By G. Alan Tarr. (Lexington, Mass.: Lexington Books, D.C. Heath, 1977. Pp. xvi + 184. $18.00.)

In this book, G. Alan Tarr explores state supreme court responses to decisions of the United States Supreme Court concerning the establishment of religion. The fundamental objective of the study is to determine what factors account for compliance and noncompliance of state supreme court decisions with theoretically determinative U.S. Supreme Court precedents. Establishment clause cases decided between 1947 and 1973 form the data base for the analysis. Compliance is defined as "proper application of standards enunciated by the Supreme Court in deciding all cases raising similar or related questions. Noncompliance involves a failure to apply—or properly apply—those standards" (p. 35).

Using 97 state decisions in which Supreme Court establishment precedents were or should have been applied, Tarr considers three sets of potentially important independent variables. First, he explores the possibility that judicial "background variables" are related to noncompliance. The overall finding is that they are not. Variables common to *all* the members of any single state court—such as state political culture, region, and state public opinion—generally are not related to court compliance rates. Further the characteristics of individual judges—such as political party, length of judicial tenure, and religion—are not related to individual propensity to vote in a compliant or noncompliant direction.

Second, the author explores the possibility that those factors which Richard Neustadt sees as leading to self-executing presidential orders are related to compliance with Supreme Court directives. Recast so as to apply to Supreme Court decisions, the Neustadtian variables explored are the finality, clarity, and persuasiveness of a decision. Using two indices constructed from various indicators, Tarr concludes that the Neustadtian hypothesis does not explain state supreme court compliance.

Finally, the author considers the hypothesis that noncompliance can be explained by the degree of disruption in existing state policies which would be required by a compliant decision. Focusing on the 40 decisions in which the state court was required to choose between noncompliance and the invalidation of state programs, Tarr finds support for the hypothesized relationship. State courts handed down compliant decisions in 90 percent of the 30 cases in which the level of disruption required by compliance would be relatively slight but in only 20 percent of the 10 cases in which the level of disruption required by compliance would be relatively great. This important result suggests that state supreme courts are greatly influenced by the political milieus in which they exist.

Another significant finding which emerges from the study is the high rate of compliance exhibited by the state courts. Of the 97 cases analyzed, state court decisions were noncompliant in only 15. This is particularly significant in that previous studies have found much higher levels of noncompliance with Supreme Court establishment decisions among local school boards, schools, and teachers.

Unfortunately this high level of compliance may also cast doubt on the generalizability of the study's other findings. The lack of relationship found in the first two areas of exploration may be at least in part a product of the lack of variance in the dependent variable. This problem is magnified for the reader by the author's failure to report fully the statistical methods employed. Thus, as is often the case, the findings of the study must be viewed with a certain degree of skepticism.

Additional caution in interpretation is mandated by the fact that the study's conceptualization and operationalization are at points quite weak. This is particularly true in the chapter dealing with background variables. For example, the indicators of public opinion are so indirect that one might question whether they should even be designated as such. The problem is further aggravated by the fact that the author's research methods are sometimes not fully explained, leaving the inquisitive reader with a somewhat uneasy feeling.

It might be unfair to close on this negative note, however. The book is well grounded in the relevant literature, addresses important questions, and produces interesting results.

ROBERT V. STOVER

University of Colorado, Boulder

Desegregation from Brown to Alexander: An Exploration of Supreme Court Strategies. By Stephen L. Wasby, Anthony A. D'Amato and Rosemary Metrailer. (Carbondale: Southern Illinois University Press, 1977. Pp. xx + 425. $19.85.)

Building an adequate theory of the Supreme Court's ability to make and implement national policy requires a fully developed description of the work of the Court. *Desegregation from Brown to Alexander,* a richly detailed analysis of the Supreme Court's response to hundreds of civil rights appeals during the Warren era, makes a very significant contribution to this effort. It will be an important resource both for those attempting to understand the Court's response to one of the most important set of policy issues in its history and to those attempting to understand the many tactical and strategic choices faced by judges dealing with a broad and potentially explosive stream of litigation touching many of the most sensitive issues in the society.

Political scientists and legal scholars have pursued a wide variety of modes of explaining the decisions of a Court which goes to great lengths to shroud its internal operations in a kind of Delphic secrecy. The approaches range from the doctrinal and historical analyses that dominate the law reviews, to the brilliant judicial biographies of Alpheus Mason, to the statistical analyses of voting patterns by David Rohde and Harold Spaeth. Much of the doctrinal and historical study concentrates on the leading cases that go into the constitutional law casebooks. Much of the behavioral research does not examine in detail the substantive legal and policy issues in the cases decided. In this study a team of researchers with very strong backgrounds in both law and political science closely examine a very wide range of decisions both in terms of the substantive issues involved and the political context and policy choices available to the Court. The resulting book is sometimes reminiscent of the work of Walter Murphy and C. Herman Pritchett, though there tends to be more detailed legal analysis and less interpretation in this study.

The book is both more and less than one might expect from the title. It is not merely a description of the school litigation from the 1954 *Brown* decision to the 1969 *Alexander* case which overturned the "deliberate speed" doctrine and required immediate desegregation in Deep South school districts. In fact, it deals with the entire range of civil rights cases, including many cases on voting, housing, the right to demonstrate, public accommodations, miscegenation, and other issues. It provides a remarkably rich tapestry, describing the circumstances and the choices in hundreds of cases which the Supreme Court chose not to hear and those which it disposed of with summary *per curiam* judgments as well as the prominent cases decided with full opinions. The massiveness of the research enterprise involved is reflected in a table of cases cited covering 16 closely printed pages.

The study, however, does not fully live up to its subtitle's promise of "An Exploration of Supreme Court Strategies." The long and detailed description of cases often tends to overwhelm the analytic framework. The pieces are all there, to be sure. Time after time the authors consider why the Court may have consistently turned aside appeals in some areas (such as school desegregation) and accepted large numbers in others (particularly public accommodations); they analyze the use and impact of different types of opinions; they consider relationships with Congress and the executive branch; they examine problems of winning lower court compliance and comment on a variety of other important matters. The problem is that the sheer volume of information and the basically chronological structure of the book tend to fragment the analysis and to prevent strong development of major analytic themes. I hope that the authors continue to draw on their understanding of this great stream of litigation to offer the profession articles pulling together their insights into the judicial process.

Readers who come to this book with an image of the Warren Court as a great crusading force for integration will go away somewhat chastened. This analysis usually shows a pattern of cautious incrementalism in developing and enforcing the law with long periods where the Court completely avoided significant cases in major areas. Strong and decisive action on schools, voting, housing, and public accommodations came only after Congress and the President acted. Throughout this long period the Court almost totally avoided the important issues of massive school and housing segregation in the North. (The first Northern school desegregation case was to be handed down four years into the Burger Court.) One comes to view the Warren Court as a center of sensitive and gradual growth of the law, only deploying its full range of authority and its many ways of reinforcing a decisive action on rare occasions, usually after national opinion had crystallized in favor of change and only localized resistance remains.

This book is not easy reading. It is packed with information and with the endless complexities of on-going struggles in the courts. This, of course, is its strength. It does not consider judicial business in neat definitive packages but captures the confusing and seemingly endless process of trying to implement change through a very powerful judicial body acutely sensitive to its environment. It could have profited from tighter editing and clearer development of major themes. Nonetheless, it will be an important resource for scholars for years to come.

GARY ORFIELD

University of Illinois, Urbana

Comparative Politics

Stability and Change in Australian Politics. By Don Aitkin. (New York: St. Martin's Press, 1977. Pp. 301. $18.95.)

Given the generalizing imperatives of comparative politics, the value to that enterprise of a study of any given country depends upon the cross-national relevance of the questions asked. Since the explanatory theory that is presumably the goal of comparative analysis must be empirically testable as well as generally applicable, such theory must be built upon data from particular contexts. Thus, country studies contribute to the goals of comparative political inquiry if they attempt to be explanatory rather than descriptive and if the phenomena they seek to explain are framed in generic rather than idiosyncratic terms. Thus, while a sound country study cannot constitute a sufficient explanation of party system stability, such studies constitute the building blocks by which such explanations are constructed.

Don Aitkin's study is an inquiry into the sources of stability of the Australian party system, the unifying theme of the book. Whether one places more value on such stability or its converse, fundamental change, surely the causes of such stability are potentially relevant to a wide range of Western democratic systems. The stability of the party system gives meaning to the wealth of survey and electoral data that Aitkin presents.

Although amply documented and discussed, the concept of party system stability is not precisely defined, perhaps because in dealing only with one clear-cut case, Aitkin was never compelled to do so. Clearly, however, Aitkin is talking about stability in terms of relative electoral support. He dismisses the various minor parties that come and go on the basis of their failure to make a dent in the major parties' monopoly on such support. Thus conceived, party system stability is a common phenomenon among Western systems; hence, this analysis of the concept is clearly relevant to comparative inquiry.

Furthermore, in studying voting and party preference, Aitkin examines the impact of several factors with broader applicability. Among these are the effect of leaders' personal qualities and their impression on voters, the impact of issues, of churchgoing, of urban versus rural residence, of class (both objective and perceived), and of class mobility. Clearly, all of these variables have an honored place in the literature of comparative politics. Yet, one could wish that Aitkin had given more attention to explicating the generic rather than the idiosyncratic aspects of his suggested explanations. Most of his efforts to draw comparative generalizations are confined to an occasional comparison with British data, probably because of their ready availability. While Aitkin notes that urban-rural differences in life style generate differences in political ideology and preference among other Western nations, he goes on to stress the unique origins and nature of Australia's Country party. The attraction, ideology and support of this party are, nevertheless, potentially relevant to an understanding of that generic class of agrarian or center parties found throughout Scandinavia. When Aitkin notes that church attendance and Labour voting are inversely related in Australia, he explains this phenomenon in terms of the attitude of Australian churches toward public morality. He thereby resists any temptation to suggest a possible cross-national inverse relation between churchgoing and left voting based upon generic characteristics of organized religion and left-wing parties. On the positive side, Aitkin carefully eschews inferences beyond the implications of the data at hand.

Many of the hypotheses Aitkin advances to account for the stability in Australia's party system may be characterized as conventional wisdom in comparative politics. The constant party preferences of most Australians despite shifts in particular issues constitutes one such variable that one should expect to find contributing to party stability in most nations. The concomitant weak impact of issues also comes as no surprise.

While Aitkin's data tends to confirm what the literature from other systems would lead one to expect, occasionally we find a new insight or an unexpected result. One of the more interesting of these is Aitkin's finding that the swing vote (individuals changing their vote from one party to another) is considerably larger than the literature about parties has suggested. However, such electoral movements tend to cancel one another; hence, when measured in the aggregate, the swing vote is small. A second is the perceptive suggestion that class-based parties are less the result than the cause of existing class consciousness.

Aitkin presents a carefully done empirical analysis of an important phenomenon, the stability of democratic party systems, albeit based upon data from only one such system. His study makes a useful and potentially important contribution to the enterprise of comparative politics, the enterprise of building empirically testable, cross-nationally valid explanatory theory. Such works as Aitkin's with its wealth of data make a far greater contribution to that enterprise than those ubiquitous quests for universally general (but empirically empty) theory most recently epitomized by Sartori's ambitious summa, *Parties and Party Systems* (London: Cambridge University Press, 1976).

LAWRENCE C. MAYER

Texas Tech University

Latin America in the International Political System. By G. Pope Atkins. (New York: Free Press, 1977. Pp. 448. $11.95.)

G. Pope Atkins has written what, in his words, is an "introductory level general survey using political science concepts" (p. xiii). In other words, what we have here is a textbook intended for undergraduate courses dealing with the role played by the nation-states of what the author identifies as the Latin American subsystem. He indicates his intention to utilize a "political systems approach" (p. xiii) and to keep "technical terms at a minimum" (p. xiv).

The book's first five chapters provide the reader with a conceptual framework, descriptions of the Latin American states, non-hemispheric states which regularly interact with those of the subsystem (the United States rates a separate chapter), and non-state actors. The last six chapters discuss regional cooperation, conflict, balance of power, and integration, as well as the Inter-American system and relations with other supranational organizations. As Atkins implies, the depth and quality of the chapters is uneven. In my view, the author is on solid ground when he describes the American role or when he outlines the major conflicts of the subsystem. On the other hand, his treatment of the internal determinants of the foreign policies of the Latin American states and his discussion of the regional balance of power leave a great deal to be desired.

Atkins' book is based almost exclusively on secondary sources, but then most textbooks are. Atkins has surveyed the existing literature diligently, as the bibliographical notes at the end of each chapter and the extensive bibliography show. Consequently, this book parallels the disciplinary strengths and weaknesses in the study of the international role of the nation-states of Latin America. In my opinion, excessive space is allocated to historical aspects of the subject; approximately one-half of the book's nearly 400 pages of text are dedicated to historical background, which leaves only 200 pages to cover all the important remaining topics. Another, less subjective, criticism is that topics such as the role and behavior of Latin American states in supra-national organizations are not documented with enough data; United

Nations voting statistics are readily and conveniently available, and could have been used in connection with the discussion contained in pp. 358–65, both as evidence and as a teaching tool. Finally, I would have preferred a more selective bibliography, one which carefully included all materials cited in the text; regretfully, that is not the case here.

I suppose that the crucial question is: would I adopt this book in my undergraduate courses in Latin American international politics? I personally would not, but other political scientists who favor a core textbook might disagree.

CARLOS A. ASTIZ

State University of New York, Albany

Comparative Legislative Reforms and Innovations. Edited by Abdo I. Baaklini and James J. Heaphey. (Albany: State University of New York Press, 1977. Pp. xvi + 353. $12.00.)

This volume is a product of the Comparative Development Studies Center, and is edited by the center's associate director and director. Although there is little that is comparative here, there is much that is diverse—targets, approaches, techniques, materials, and the authors, who represent a variety of affiliations.

The targets are also varied. Three pieces focus on Congress, four on state legislatures, and four more on legislatures in other countries—the chambers of deputies of Brazil and Lebanon and the parliaments of Denmark and India. Approaches range from the strenuously systematic on the one hand to the informed impressionistic on the other. Some focus narrowly and intensively, others survey a wide terrain. The historical, geographical, and methodological diversity is somewhat overwhelming and the volume, therefore, seems to be less than the sum of its parts. Some of the parts are quite good.

At a general level is Baaklini's survey of legislative reform in Brazil, where a strong executive, based upon a military-technocratic alliance, dominates the Chamber of Deputies. Given the political system, what difference can changes in legislative organization and procedures and managerial and technical innovations possibly make? At least some differences, says Baaklini, because the legislature offers a platform and rallying point for opposition to the regime. Whatever the odds against it, the Brazilian legislature, "has demonstrated a phoenix-like quality of survival" (p. 259).

Congress, in contrast, has not merely survived; it has prospered, growing fat and powerful. For those concerned with legislative control, the Johannes study of statutory reporting requirements merits close scrutiny. Executive agencies furnish Congress with more than 1000 reports a year, and the author examines their types, targets, purposes, use, and effectiveness. For those intrigued by the institutional development of Congress and particularly its expanding resources, Beckman's review of the service of the Congressional Research Service is instructive. CRS provides public policy research and information assistance, responding to about 250,000 requests a year as well as proposing subject and policy areas for committee analysis, preparing lists of programs scheduled to terminate, holding public policy seminars, developing and providing automated information services, and dealing with the Office of Technology Assessment and the General Accounting Office, two other instrumentalities of Congress.

State legislatures have made considerable progress in recent years. Hedlund and Hamm measure the effects of several rules changes on the decision-making performance of the Wisconsin Assembly. Their four dimensions of performance (productivity, expeditiousness, efficiency, and adaptiveness) and their 23 operational measures (items like the number of bills reported divided by the number referred to a committee and the number of days in session divided by the number of bills passed) will not persuade everyone, but they do permit illuminating analysis. Crane's examination of new techniques of legislative oversight is also worthwhile. His assertion that performance auditing and program evaluation "make it possible to specify program objectives, develop indicators and standards by which results can be measured, compare actual performance with promise" (p. 118) is surely wishful thinking. But his efforts to distinguish among types of program review and his discussion of factors that promote success are informative indeed. The shortest piece in the volume, by Nunez and Whorton, is most penetrating. It comments on a legislature's problem in anticipating just what is involved in administering its laws. The authors propose the creation of an "administrative impact statement" and the establishment of an "administrative feasibility unit," which would introduce new issues into the legislative process.

Comparative Legislative Reforms and Innovations covers much ground and moves in many directions. Since the only unifying force may appear to be the binding affixed by the State University of New York Press, the timid might be unnerved. But for the hearty, who delight in the study of legislatures, there is more than enough in the volume to command interest and capture attention.

ALAN ROSENTHAL

Rutgers University

Quantum Dokumentation: Quantitative Historische Forschung 1977. By Wolfgang Bick, Paul J. Müller, and Herbert Reinke. Historisch-Sozialwissenschaftliche Forschungen (HSF), Vol. 1. (Stuttgart: Ernst Klett Verlag, 1977. Pp. 236. DM 39.)

Die Analyse Prozess-produzierter Daten. Edited by Paul J. Müller. Historisch-Sozialwissenschaftliche Forschungen (HSF), Vol. 2. (Stuttgart: Klett-Cotta, 1977. Pp. 226. DM 29.)

Quantitative Methoden in der historisch-sozialwissenschaftlichen Forschung. Edited by Heinrich Best and Reinhard Mann. Historisch-Sozialwissenschaftliche Forschungen (HSF), Vol. 3. (Stuttgart: Klett-Cotta, 1977. Pp. 253. DM 39.)

Faschismus als soziale Bewegung, Deutschland und Italien im Vergleich. Edited by Wolfgang Schieder. (Hamburg: Hoffmann & Campe, 1976. Pp. 208. DM 24.80.)

Der Aufstieg der NSDAP in Mittel- und Oberfranken (1925–1933). By Rainer Hambrecht. (Nürnberg: Schriftenreihe des Stadtarchivs, 1976. Pp. 596. Price not given.)

Here is a brief glimpse of one of the most rapidly growing interdisciplinary areas of European social science research of great potential impact on the study of politics, especially of political history. In just a few years, the Cologne Committee for Quantification in Historical-Social-Science Research (QUANTUM) has given a major impetus to the methodological convergence of the quantitative study of history, particularly of social history, with sociology, social psychology, anthropology and political science. A series of QUANTUM publications has taken stock of the developments in order to inform the interested public about the projects, names of researchers, and themes of conferences in this area. The ripples of this new wave and the growing methodological rigor can be observed in many areas of historical research, including the two books discussed below.

The first volume of the series, *Historisch-Sozialwissenschaftliche Forschungen* (HSF), contains the results of a QUANTUM survey of ongoing and projected research of this genre in Germany, Austria, and Switzerland, as well as of related projects on German subjects in the United States, Canada, and other countries. Although it can hardly be called exhaustive, especially not outside the German-speaking areas, this survey shows the extraordinary range of current quantohistorical social research over the historical periods and over topics ranging from demography, crime, and medical history to controversial topics of the sociopolitical history of industrialization, the social composition of fascist movements and electoral history. The editors also comment on their survey and present statistics on the financing, nature and prevalent themes of the projects, as well as on the methods and scientific training of the researchers. The book's chief merit appears to lie in having brought the materials together and ordered them in a preliminary way.

The second HSF volume deals with the analysis of "process-produced data," that is, data generated by organizations and systems in the course of their normal functioning rather than census results or surveys. It is a collection of sociological articles assessing the limitations and possibilities of such sources in the historical study of public, judicial and police administration and in an examination of the patterns of mobility and social circumstances of nineteenth-century factory workers. The contributors are rather critical of the unreflected use of masses of such data "as if a specific hypothesis had to be verified against a snapshot of reality" (p. 7). The current use of process-produced data is placed instead in the context of the history of social research where it represents, let us hope, a more sophisticated return to earlier sources. Today micro-data can be related to one another in ways undreamed of in the age before public opinion research.

A third volume contains essays on quantitative applications in the areas most typical of the change of directions in recent German historiography: changes in social structure, living conditions of the lower classes, and the early history of trade unions and other associations. The themes are familiar from recent Anglo-American, French, and Scandinavian studies. The methods include time series analysis, thematic mapping, and the quantitative analysis of masses of biographical data. Further volumes have reported conferences on quantitative research on the economic and social history of late medieval and early modern Europe and on the social composition of interwar fascist movements.

The relevance of these quantohistorical social-scientific approaches to contemporary political science is, of course, limited by the availability of reliable quantitative data on many subjects and particularly on prebureaucratic periods and societies. Their greatest contribution so far, in my opinion, lies in illuminating certain highly controversial issues of political economy, social-structural analysis, and social-psychological motivation, such as currently agitate the great debates among European social scientists and historians. The data-poor and theory-rich state of these debates, moreover, has spurred the inventiveness of quantitative researchers remarkably and lends to the ongoing discussion a creative tension and topical poignancy one can only wish would inform more of the quantitative social science research in this country.

Among recent publications in one of the foci of European controversy is a collection of articles edited by the German historian Wolfgang Schieder, entitled (in translation) *Fascism As a Social Movement,* which deals, among other things, with the current state of the "class analysis" of Italian fascism and German national socialism. Some of the chapters represent a new departure toward putting a seemingly saturated area of scholarly writing to the empirical test of quantitative investigations and point up at once the possibilities and limitations of such measurements against a background of intense ideological controversy over the nature of fascism. The editor agrees with Renzo de Felice, Karl D. Bracher, and others that German and Italian fascism may not have had much in common anyway. The overheated debates over the "theory of fascism," on the other hand, clearly imply a sameness of fascisms everywhere, with minor idiosyncratic exceptions, and postulate for it a uniform social basis, described with disarming vagueness as "the ruling classes," the bourgeoisie, the lower middle class, or a combination of old and new middle classes with the peasantry (pp. 97–118).

At this point, quantitative evidence is marshaled to demonstrate to what extent the membership or leadership of the early Nazi or Italian fascist parties conformed to this description. Thus the annoying presence of workers in these pre-takeover movements needs to be explained away, for example, by characterizing proletarian early Nazis as probably (1) rural and hence "closer to a *Mittelstand* mentality," or (2) unemployed or fearful of becoming *Lumpen* proletarians (p. 31). The large number of workers among European fascist movements indeed is hard to explain if one insists on construing the political motivations of these people primarily from their occupations. With Nazi leadership cadres and especially with the waves of opportunistic new members and supporters after the takeover, when the new and old German elites coopted each other on the other hand, a prevalence of middle- and upper-class occupations is easy to demonstrate. But after all this stands revealed, the question still remains: "Why did they do it?" An intensely political movement like the NSDAP can hardly be exhausted in its political motivation with occupational analyses alone.

The complexities of the Italian development, particularly bourgeois-urban and agrarian fascism and the alliance of fascist syndicalism with the extremist wing within the PNF, resist easy generalizations about the monosocial basis of Italian fascism even more than does the early NSDAP. Was the Italian movement based solely on "monopoly capitalism," the *ceto medio,* or the rebellion of the new against the old middle classes, or was it a catch-all movement of varied elements? As Schieder points out, the data base on the Italian fascists is rather poor and, here too, the changes in social composition before 1919–1922 and after Mussolini's takeover were considerable. Agricultural and industrial workers made up a substantial minority (39.7 percent) although frequently motivated by social pressures and expectations of jobs (pp. 76–77, 81–82). In some places and during certain phases, the PNF may indeed have been purely the instrument of the class interest of landowners and industrialists. But the overall heterogeneity of the early years, the maverick course of Mussolini, and the pronounced shift to the right after 1922 again make generalizations about the early period hazardous. Mussolini's massive cooptation of bourgeois and Nationalist elements in 1923 makes the earlier PNF look social-revolutionary and radical by comparison.

What has been the impact of the new quantohistorical approaches on the more conventional historical research on related topics? A good example is a 1975 doctoral dissertation, now available in book form, entitled (in translation), *The Rise of the NSDAP in Middle and Upper Franconia,* by Rainer Hambrecht. One of the recent additions to a growing regional historiography on the rise of national socialism, this book could hardly avoid the historian's conventional preoccupation with personalities, events, situations, and beliefs in favor of a facile manipulation of macropolitical data. The author indeed accounts for these in Julius Streicher's bailiwick in exemplary fashion. But then he also exhibits an overriding awareness of quantitative relationships which places him in the context of the literature reviewed above.

Without comspicuous tables, the author begins with a quantitatively secured social and economic history of the region, explores the social composition of the NSDAP in relation to the population of the region, and skilfully analyzes the elections there at all levels. There emerges an integrated picture of the party's growth in this (until 1929) key region and a sophisticated, critical assessment of the impact of the Depression and of the mobilization of the landslides of 1930 and 1932.

Hambrecht's reliance on objective facts and figures and the breadth of his integrated approach in the end supply balanced answers to the old question, "Why did they do it?" Above all, the answers are political and ideological rather than economic class interpretations of the Nazi onslaught. The movement in Middle Franconia, at least, was clearly the child of earlier antisemitic groups which in the early thirties succeeded in taking over the following of existing bourgeois, liberal-conservative parties and veterans' groups. Its rising vote benefited particularly from its farm propaganda in Protestant areas, while its membership continued to be dominated by the old and new middle classes (especially teachers and civil servants), including 10 percent farmers. The share of workers rose to 40.5 percent of whom only about one seventh were unemployed. Workers were even more numerous among the stormtroopers (SA). While no one will claim that the victorious NSDAP was a workers' movement, the label of a "catch-all" party appears to be well deserved.

These books by Hambrecht and Schieder show the extent to which quantohistorical social science approaches are making themselves felt in the literature on current history. Their relevance to political science requires no demonstration.

PETER H. MERKL

University of California, Santa Barbara

Voting and Party Competition: A Theoretical Critique and Synthesis Applied to Surveys from Ten Democracies. By Ian Budge and Dennis Farlie. (New York: John Wiley, 1977. Pp. x + 555. $36.50.)

This is a book of some importance for students of electoral behavior and of comparative politics generally. Its aims are highly ambitious: to review and synthesize leading theories about the behavior of voters and parties, and to test a number of propositions with survey data (plus some party platforms and manifestoes) from 22 elections in 10 countries.

The scope of the undertaking and the thorough—not to say tedious—manner in which the authors go at it, make the length of the book (and unfortunately its price as well) quite formidable. Some readers may wish to follow the authors' suggestion, p. 5, and take only a quick look at the theories, moving directly to the second half of the book, which presents empirical findings and contains most of the original contributions.

The first half has value as well, however. After brief discussions of social background and party loyalties, the authors launch into detailed discussions of several rational choice-oriented theories: Downs' theory (his general reasoning carefully distinguished from his unidimensional spatial model); the Davis, Hinich and Ordeshook multidimensional spatial models; Chapman's theory of distributional allocations in a unidimensional space with party biases; and Robertson's "party reserved" space with modified office-seeking assumptions. The coverage of the review is slanted somewhat toward the work of British scholars, ignoring some recent developments in the United States. But it is for the most part well informed, technically sound (with occasional lapses, as on the nature of plurality-maximizing assumptions), and full of sensible observations concerning plausibility. The lists of assumptions and implications of each theory are particularly useful.

The synthesis draws heavily upon David Robertson's *Theory of Party Competition* (1976), and resembles arguments which have been arrived at independently by a number of scholars in the rational choice tradition. A pure policy space with vote-seeking politicians and policy-voting citizens is rejected. Long-term party loyalties, related to demographic characteristics, are argued to arise because of information costs, and to be supplemented by short-term electoral cues. Politicians are asserted to have policy goals of their own and also to appeal to party activists as well as ordinary voters, so that parties converge or diverge under various conditions. It is well worth looking at the brief statement of the synthesis on pp. 497–500, and the more formal (though not deductive) statement on pp. 238–49.

The empirical half of the book is based upon a more or less Bayesian technique involving a "likelihood ratio space," familiarly referred to as "LiRaS," which—despite lengthy explanations in the text and appendices—is likely to confuse many readers. Contrary to the authors' claims, the log likelihood ratios have rather little to do with "spaces"; they are simply indices of the probability of voting for a given party or *tendance*, or of not voting, based on whatever set of independent variables is used to compute them. Assertions throughout the book about "locations" or "closeness" in a space really refer to the *distinctiveness* of party voters with respect to the independent variables. The "information values" of independent variables are measures of their predictive importance, not unlike standardized regression coefficients for nominal variables, but with some peculiarities—such as being inflated by multicollinearity and including no interaction terms.

When all is said and done, then, the empirical effort mainly involves predicting votes in the 22 surveys by whatever independent variables are available. These are subdivided into "predispositions" (demographic characteristics, with party identification fortunately excluded as potentially tautological), and "cues" (issues and candidate preferences). The chief focus is on the relative importance of cues and predispositions, within and across countries and elections.

Many of the central findings are given in chapters 8 and 9. The overall success in predicting both aggregate (pp. 264–65) and individual (p. 277) votes looks quite respectable, although nonvoting is not well accounted for outside the U.S. Cues seem to predict votes slightly better than do predispositions, especially in the U.S., Canada, and Britain. Sweden, the Netherlands, and Germany are more dominated by predispositions. These comparisons continue to hold up (p. 347) when "long-term" issues are reclassified as predispositions. Predictive stability is lower for countries and elections with high cue-dominance or with many voters in the "middle," i.e., without strong predictions of party vote. Class cleavages are important everywhere (p. 352), especially in Sweden, Britain, and Australia, although, as the authors acknowledge, their importance (e.g., relative to religion) is probably overstated by grouping parties into class-based *tendances.*

Certain weaknesses in these findings become apparent in chapter 9, which lists the specific variables from the various surveys. There is a great deal of unevenness in the available data. In some cases, e.g., the omission of measures of language, religion, or province for the 1968 Canadian election, lack of data seriously affects estimates of the relative importance of cues and predispositions. Whatever is well measured (religious "issues" in Canada) has an advantage in appearing important. The imbalance in available data is most obvious in the cases of Canada (p. 310), France (p. 313), and the U.S. (pp. 334–41–where, as with Canada, it affects overtime as well as cross-national comparisons), but it casts at least some doubt on all the findings.

In addition, despite considerable agonizing about it, the knotty question of causal priority between issues and stable partisanship is not resolved, which undermines the main objective. Nor, for that matter, is it always obvious that the "independent" variables are totally independent. The cue/predisposition distinction itself suffers from a failure to separate voters' preferences (e.g., for certain leadership characteristics), which may be either short- or long-term, from the stimuli in particular elections (e.g., a candidate's personality), which may also vary or stay the same from one election to another.

There is some interesting treatment of electoral stimuli, under the rubric of "leaders' parties," in chapter 11. Budge and Farlie adapt Robertson's analysis of British party manifestoes to the LiRaS methodology and extend it to include U.S. party platforms. The resulting charts (pp. 424–25) of the magnitude of party differences over time are intriguing. There are distinct party differences in both countries but, as one would expect, the British parties are generally a little farther apart than the American. In both nations, party differences are smaller before close elections than before landslides: Budge and Farlie assume that anticipation of a close outcome causes politicians to compete for votes and converge, but the causation could as plausibly run the other way; close parties may cause close elections. Similarly, it is not clear whether the fact that politicians emphasize the same issues that predict votes (pp. 435–36) demonstrates opinion leadership, as the authors claim, or whether it reflects responsiveness to what the voters want to hear, as in most rational choice theories.

In a brief review it is impossible even to allude to all the material in this bulky book. *Voting and Party Competition* may not offer very exciting reading, but it is full of food for thought and is worth the effort of working through.

BENJAMIN I. PAGE

University of Chicago

The Black Homelands of South Africa: The Political and Economic Development of Bophuthatswana and KwaZulu. Perspectives on Southern Africa No. 21. By Jeffrey Butler, Robert I. Rotberg, and John Adams. (Berkeley: University of California Press, 1977. Pp. x + 250. $12.50.)

This remarkable book, written by an historian, a political scientist, and an economist, is yet another welcome addition to the stellar series of studies on Southern Africa produced by the University of California Press since 1971. The price, given the rampaging inflation, is quite attractive relative to the size of the book, and the text is supplemented by two figures, three maps, twenty tables, a fourteen-page index which is both thorough and reliable, and a four-page selected bibliography. Although one can always quibble about the choices cited in such a bibliography (because the criteria for inclusion are often never made explicit), its one glaring deficiency is the authors' failure to give the publisher of books; this is a serious oversight because many of the titles are published in South Africa, rather than in the United States or the United Kingdom.

The preface (pp. ix–x) reveals the wide range of institutions and persons in both the United States and South Africa the authors consulted. The many sources cited, ranging from the late B.S. Biko to the white technocrats in Pretoria who are devoted disciples of the separate development doctrine, show that the authors made a conscious effort to consult all shades of opinion. The authors are to be commended for their use of original newspaper sources, both in Afrikaans and in English, and the extremely important bilingual *Mafeking Mail and Botswana Guardian.* The use of original sources is refreshing, given the number of works on South Africa that rely merely upon press digests and summaries.

In light of South Africa's 1976 grant of independence to the black homeland of the Transkei, the November 30, 1977 South African general election (among the South African whites), and the announced date of December 6, 1977 for the independence of Bophuthatswana (the homeland for Tswana-speaking Africans), this book is unusually topical. Moreover, it is an accurate and well-documented account of the two (Tswana- and Zulu-speaking) African homelands written within a larger framework of inquiry that could easily be utilized to examine the other homeland areas. The authors write lucidly and engagingly about the challenges to the core white body politic posed by these two black peripheral subpolities, and they guide the reader effortlessly through the rudiments of African economic development. One need not be thoroughly grounded in the precepts and arcane jargon of developmental economics to appreciate the immediacy, the gravity, and the moral implications of the economic burdens facing the homelands' leaders and their inhabitants, both permanent and transient. One can see how the maladies of the homelands relate to the patterns of growth of the metropolitan white economy. The persistence of African migratory labor is only one of the more glaring manifestations of the links between the two economies which, as the authors demonstrate very convincingly, are heavily asymmetrical. Much of the core-periphery analysis, moreover, could be applied to South African relations with neighboring Lesotho, Swaziland, and Botswana, with particular reference to migratory labor patterns.

One of the most significant contributions the authors make to the serious study of Southern African politics and regionalism is their subtle, yet devastating, juxtaposition of the promises and the actual rewards of the system of separate development. The careful delineation of the gap between the ideology of separate development (as developed by Africaners as an exercise in civil religion but practiced adroitly by English-speaking whites intent on retaining the perquisites of white hegemony) and the actual delivery of the political goods to the African inhabitants of the homelands can have a greater and longer-lasting impact on the decision makers of the South African political class than the more dramatic and rhetorical gestures of the African states in the Organization of African Unity and the United Nations. It is thus a book that deserves a wide audience both within and outside the groves of academe, and the cognitive map it provides will be of great interest to those responsible for Western and African statecraft and for those who are no longer content with simplistic analyses of, and mindless cant about, the human problem called South Africa.

RICHARD DALE

Southern Illinois University, Carbondale

Revolution in the Third World: Myths and Prospects. By Gérard Chaliand. Translated by Diana Johnstone. (New York: Viking Press, 1977. Pp. xxii + 195. $11.95.)

This book first appeared in France in 1976 as the *Mythes Revolutionnaires du Tiers Monde.* Chaliand has had an impressive background of journalistic involvement in the causes which he surveys in this work: in the early 1960s he had served as editor in chief of the Algerian weekly, *Revolution Africaine*, and traveled in Africa, Cuba and the Middle East. Among other books, he has written *Peasants of North Vietnam* (1970) and *The Palestinian Resistance* (1972).

This latest work ranges over a very broad horizon, both geographically and thematically. It examines the revolutionary experience, the potential, and the revolutionary prospects of many countries of Africa, Asia and Latin America, all in a series of short essays each dealing with a different aspect of revolution.

One motive which informs most of the essays is an attempt to make a realistic reassessment of the failure of revolution throughout the Third World. Chaliand perceives this failure in several ways. Many revolutionary attempts failed to dislodge the forces of *status quo* because of such causes as ineffective organization, lack of grass-roots support, inadequate leadership capabilities of the revolutionaries, and in general terms, a lack of realism about the situations of the societies whose transformations the revolutionists sought to achieve. There is an even more substantial discussion of the failure of revolutionary groups and causes to consummate far-reaching social, economic and cultural changes when the opportunities of power have ostensibly been theirs. Chaliand deplores the tendency of all successful revolutions to bureaucratic ossification, even in cases like China, Yugoslavia and North Vietnam (the latter being the apparently least offensive case of bureaucratization, in the author's view). In some instances, as in Allende's Chile, he regrets "too many reforms and not enough revolution" (p. 128).

A more general phenomenon which Chaliand diagnoses and deplores is the tendency of Third World revolutions to adopt a nationalist character, directed against imperialism and its domestic affiliates and agencies, but lacking a social orientation which would allow for the redistribution of wealth and power to the masses. The discussions of the Egyptian case after 1952 and the Algerian after 1962 illustrate the gap between "socialist" rhetoric of leaders and the realities of bourgeois and bureaucratic privileges and hierarchic stratification of post-revolutionary societies. In both instances, as in many other cases in Africa and Asia, the masses have not been mobilized into revolutionary action but rather "kept in check" in order to forestall and control any further social, economic or political change. The revolutionary regimes tend to stagnate and the masses, as in Peru, under military auspices in the sixties and seventies, are generally "led by the nose," that is, manipulated by leaders rather than involved in decision making.

Chaliand's critique presupposes, of course, that "revolution" is somehow a desirable phenomenon in social development, and it also presupposes some criteria by which, presumably, one might judge the merits of revolutionary upheavals: what is attempted and what is accomplished. The need for revolution in this book hinges on the connection between the evils of under-development (including hunger, poverty, inequality and oppression) and "dependence," or domination by the industrialized Western capitalist nations. Unfortunately, the author tends to take this nexus for granted. A brief discussion of about six pages entitled "A World Both Poorer and Less White" (pp. 10–17) is logically flawed. A rather skimpy enumeration of some of the problems of Third World countries is concluded by the sweeping assertion that "in short, the industrialized capitalist states control Asia, Africa and Latin America to their own profit. Their control keeps production, political and ideological relationships static, and development becomes impossible" (p. 15). Elsewhere, the author actually contradicts this sweeping judgment with references to the allegedly successful development of Brazil, and Iran, among others (e.g., p. 185). Moreover, the "static condition" which he alleges to be the result of capitalist control of the Third World is by no means self-evident from the "tale of turmoil" unfolded in this book.

It is only at the end of the work that we are given explicit criteria of revolutionism. While Chaliand confesses that "it is time to announce the death of Utopia" (p. 194) as embodied for different people at various times by China and the USSR, he asks if the objectives of socialism, to wit, "the abolition of conflict and the end of history" are worth defending. And he concludes that "the task remains to challenge the mythologies of the nation state, the cult of work, the submission to authority, the imposture of groups and parties who claim to possess the truth . . . with a view to satisfying all basic needs and eventually attaining the supreme luxury: free time" (p. 195). How all of this is genuinely relevant to regions in which, according to the author's figures, 70 percent of the world's population receive 22 percent of the world's income (p. 10) is not readily evident.

ALEXANDER J. GROTH

University of California, Davis

The Man Who Lost China: The First Full Biography of Chiang Kai-shek. Brian Crozier, with Eric Chou. (New York: Charles Scribner's Sons, 1976. Pp. xv + 430. $12.95.)

The title of this work is a bit misleading since it was not that the late President Chiang "lost China," but rather than the Japanese invasion forced him to retreat inland and in turn paved the way for Chinese Communist advances. Chairman Mao himself later told visitors that the Japanese invasion helped defeat Chiang Kai-shek and the Nationalists. Even at the time, in 1937, many observers thought the Japanese were assisting the cause of the Chinese Communists. Berlin warned Tokyo that its invasion would "help the Chinese Communists." Consequently, Crozier's book is more an account of the Republic of China from 1911 to 1975 rather than a full biography of President Chiang, although the author makes some attempt at appraising Chiang's character.

The book starts with a section entitled, "The Enigma of Chiang Kai-shek" which discusses both his conservatism and his revolutionary goals. Then the second section, which details the 50-year period from 1887 to 1937, describes Chiang's military training in Japan, his role in the 1911 Republican revolution and Chiang's disagreement with Sun Yat-sen over the decisions to collaborate with the Soviet Union in 1923–24. In short, well-written chapters, Crozier describes major events such as Chiang's leadership of the famous Northern Expedition of 1926–28, during which he expelled both the Soviet advisors and the Chinese Communists from the Nationalist Party. Other chapters in this section discuss Chiang's marriage to the American-educated Soong Mayling, the Christian daughter of a wealthy Shanghai commercial family, and Chiang's superficial cooperation with the remaining Chinese warlords after establishing the Nationalist capital in Nanking. Crozier deals with the efforts at nation-building during the "strenuous decade" from 1927 to 1937 and also discusses the five extermination campaigns during which the Nationalist armies slowly tightened a noose around the Chinese Communist base areas in central China, forcing the Communists to undertake the Long March to Yenan.

Part 3 discusses the world war and the last round of the Chinese civil war in the decade from 1939 to 1949. Crozier concludes that Chiang Kai-shek believed that preserving unity was the best way to create and defend China, but that he was unable to rally the warlords sufficiently in the late 1930s. Crozier also states that the Nationalists ignored the peasantry who formed the vast majority of the Chinese population, and claims that Mao Tse-tung was a more astute strategist than Chiang Kai-shek. Crozier, however, admits that after 1949 Mao's policies resulted in the death of millions who were labeled as class enemies. Crozier's discussion of the Marshall Mission in the postwar period is quite good. He shows how the Communists and the Nationalists could never collaborate except on a temporary basis.

For his material Crozier draws heavily upon the experience of his collaborator, Eric Chou, a Chinese journalist who was active during the war years. Crozier also takes many of his details from the biography of Hollington Tong and scholarly studies such as those of Paul Sih. Crozier, who has written a number of popular histories and biographies, visited Taiwan in 1957 and again in 1974.

The fourth section discusses the accomplishments of Chiang Kai-shek after the Nationalists withdrew to the island of Taiwan. Crozier finds that on Taiwan the Nationalists achieved tremendous economic growth and social unity in the years from 1949 to 1975. Chiang Kai-shek's health declined after 1972 and his son, Ching-kuo, became premier of the government on Taiwan. The transfer of power was fully recognized in May 1978 when Chiang Ching-kuo was inaugurated as the president of the Republic of China, the same post his father had held. In his treatment of recent years, Crozier describes the Kissinger-Nixon approach to Peking which so angered Taipei. The book was published before President Carter's 1976 election and so does not discuss his intention to fulfill Nixon's "Shanghai communique" to normalize relations.

The work has many laudable features as a history of the Republic of China from its origins, through its struggle to gain and retain control of the mainland, and during its years of refuge on Taiwan. For those interested in a record of the Nationalists, rather than a more intimate personal biography of President Chiang, the work serves a good purpose.

DOUGLAS H. MENDEL, JR.

University of Wisconsin, Milwaukee

Wartime. By Milovan Djilas. Translated by Michael B. Petrovich. (New York and London: Harcourt Brace Jovanovich, 1977. Pp. x + 470. $14.95.)

This third volume of the author's memoirs covers the World War II years. It is a story of the Communist-led Partisan movement in Yugoslavia (as seen by Djilas), seeking to avoid annihilation by punitive Axis expeditions while at the same time carrying on a civil war against the Mihailovich Chetnik guerrilla movement which they perceived to be the principal barrier to ultimate Communist victory. Djilas confirms a number of things that we have known for some time: that the Partisans were extremely weak in Serbia even at the end of the war; that they could not have taken Belgrade in October 1944 without the Red Army (they did not find *one* party member there when they entered the capital); that the Partisans had reached agreements with the Nazis so as to be free to fight the Chetniks; that the Communists hid their real aims behind a popular front; and that their difficulties with the Russians began early.

What is new is the recitation of extensive wartime excesses and cruelties that the Partisans committed—executions of countless men who personally were not guilty of any crime, the burning of houses and villages, and the execution of prisoners on the spot (even some who had surrendered on the basis of a Communist promise that they would be tried after the war)—subjects which are taboo in Yugoslavia, although the Yugoslavs have said much about atrocities by others. Djilas also tells of the postwar liquidations of captured or repatriated opponents of the Partisans, which he depicts as "senseless acts of wrathful retribution" (p. 447). He admits, however, that given the Communist party's "power structure and the chain of command, no one could have carried out such a major undertaking without approval from the top" (p. 449). In emigré circles Djilas is blamed for some early wartime atrocities in Montenegro, for which he disclaims responsibility.

Moreover, the book makes clear that the Partisans rarely sought to engage the Nazi forces in combat; they were more or less constantly on the move, seeking to escape annihilation by German troops. At other times they were attempting to strike mortal blows against the Chetniks. Of interest is the author's admission that there were few workers in Partisan ranks, which were made up mainly of Serbian peasants who had fled to the mountains to escape the fate of fellow Serbs who were massacred by the Nazi-puppet Ustashi regime in Croatia. Also interesting is the fact that the Partisans resorted to requisitioning food and confiscating livestock, but that after the war the Communists did not honor even written promises to pay. A combination of factors (including Allied aid) contributed to ultimate Communist victory, but their singleness of purpose and absolute determination played a significant role.

Here and there are some further revelations about Yugoslavia's relations with the Russians, including a remark to Djilas by Soviet General Korotayev that when Communism is victorious the world over, then wars will be fought with the ultimate bitterness. Djilas also sheds further light on how quickly Communist leaders demonstrated their desire for luxurious living and special privileges. Even before power was achieved, the struggle for material things was on, and sensitivity about rank, title, and position was exhibited in various contexts.

One gets the impression that part of Djilas' narrative constitutes a kind of settling of accounts with a number of his former comrades, some dead and some still alive. Many members of the Communist party Central Committee (and even the Politburo), usually portrayed as faultless and self-sacrificing, are shown by Djilas to have had serious weaknesses, possessed by vanity, and other less than honorable character traits. For the first time in his writings, Djilas says a number of uncomplimentary things about Tito—he mentions his nervousness, his rashness, his doubts and inconsistencies, and at times his unfairness. Djilas also reports in some detail how Tito's needs were taken care of by their taking a cow along so that he would have milk and that a mistress was always with him.

Over the past 20 years, this erstwhile Communist revolutionary has said a number of things—and said them well in his starkly dramatic style—about Yugoslavia, and about Communism in general, which have contributed significantly to our knowledge of Communist affiars. While it can safely be assumed that Djilas is not telling all, no serious student of Yugoslavia's wartime years can ignore this book.

ALEX N. DRAGNICH

Vanderbilt University

Electoral Politics at the Local Level in the German Federal Republic. By Linda L. Dolive. (Gainesville: University Presses of Florida, 1976. Pp. 110. $4.00, paper.)

This publication has the welcome purpose of closing part of the gap in our knowledge about local voting patterns in West Germany, their relationship to regional and local elections, and their relevance to political parties. The book is consciously comparative, and frequent references are made to French, Swiss and Scandinavian studies, as well as to numerous works in German and English on local government in West Germany.

Linda Dolive begins with a brief review of West German parties and election results at the national and state (*Land*) levels and moves quickly to a discussion of the local level. She notes the "scanty" information about voting behavior and political parties at the local level everywhere and points to the difficulty of making generalizations about political parties when we know so little about their activities and effect at the base of the political system, the local community.

Following an outline of local government structure and "the style of local politics," Dolive turns to the methodology which she has employed to deal with the approximately 24,500 municipalities (*Gemeinden*) which existed before the wide-ranging territorial reforms in the various states were introduced in the late 1960s and 1970s. She took a 20 percent sample of the 588 *Gemeinden* of the Rhineland-Palatinate and focused on elections held in these *Gemeinden* from 1947 to 1969. The remainder of the study contains chapters on electoral politics at the local level, the politicization of local elections, electoral participation, national parties and their local electorates, the development of party systems, and the relationship of parties to local electoral behavior.

There are, in my view, two very serious problems with this study. The first is related to the selection of the Rhineland-Palatinate as a "representative" *Land,* which it is not, and to the sample of Gemeinden which was drawn from this *Land.* The author implies throughout the monograph and on occasion states without qualification that her findings are typical of *Gemeinden* in general, when in fact they apply mostly to the large majority of small villages up to 1500–3000 inhabitants in which only a *minority* of Germans live. In 1965, for example, 90 percent of the 24,500 *Gemeinden* in Germany had fewer than 3000 inhabitants; but only 26 percent of the population *lived* in these villages! The failure to distinguish clearly between *Gemeinde* size leads to some unacceptable conclusions about non-partisan local elections. What is important to note is that at least three-fourths of the West German population *does* vote in municipal elections which are generally partisan-oriented. Furthermore, this proportion has increased under the impact of local territorial reforms which have virtually eliminated all villages with fewer than 4,500–5,000 inhabitants as autonomous legal structures.

The second serious problem is the exclusion of counties from the definition of "local" elections. There is no explanation for this, in spite of the title of the monograph which certainly suggests to me that counties will be considered. If, in fact, counties had been included, Dolive would have recognized very quickly that her generalizations apply almost exclusively to villages. Elections for county councils, held simultaneously with *Gemeinde* elections, are strongly dominated by the established parties.

There are also a number of relatively minor inaccuracies, omissions or interpretations that call for comment. The office of city manager in North-Rhine Westphalia, Lower Saxony and Hesse may have been "non-partisan" (p. 14) in conception; in actual practice it has become a position of the utmost partisan political importance. The discussion of local tax sources (p. 15) is largely obsolete, these having been changed substantially in the finance reform of 1969. It would have been very useful if Dolive had explained in some detail the distinction between PR and plurality elections in German localities. And to interpret state and national electoral trends in Germany as a movement toward a two-party system is not without its risks. What we have at the moment are two "full" parties (CDU and SPD); one "half" party (FDP), which is strongly oriented toward the SPD as a coalition partner; and another "half" or "one-third" party (CSU) which, though *generally* allied with the CDU, can and does threaten to turn the West German party system into a four-party system.

Dolive writes clearly and well, and, with the exception of failing to control for size of *Gemeinden,* has utilized numerous statistical techniques to good advantage. In spite of the two serious weaknesses discussed above, I would recommend the monograph for courses in comparative local politics and for libraries with strong collections on Western European government and politics. It has no index, but the subdivisions within the chapters compensate for this lack. It has a good bibliography.

ARTHUR B. GUNLICKS

University of Richmond

The Poverty of Revolution: The State and the Urban Poor in Mexico. By Susan Eckstein. (Princeton, N.J.: Princeton University Press, 1977. Pp. xv + 300. $17.50.)

Sociologist Eckstein's thesis springs clearly from her own words: "To protect primarily the interests of capital and, to a lesser extent, the interests of the salaried 'middle class' and organized working class, the Mexican state has had to limit demands by urban and rural poor" (p. 25). Writing from a Marxist perspective, she focuses on three lower/lower middle-class Mexico City neighborhoods and their various links to the state. This volume provides useful information about the social, economic and political organization of these neighborhoods and the mechanisms employed by the government and the dominant PRI party to integrate their residents into the political system. The book suffers, however, from Eckstein's failure to provide important (and apparently available) data needed to support her argument.

The first chapter reviews thoroughly and compellingly a huge body of literature on the role of the Mexican state in the maintenance of wealth and income inequality and the facilitation of private capital since the revolution. A second chapter describes the history and nature of the three communities—a center-city tenement area, a suburban land invasion, and a government low-cost housing project. A lack of statistical data from interviews prevents an evaluation of the residents' true poverty in the Mexican context, a serious flaw since Eckstein admits that many are not poor, but lower middle-class or above. Chapters 3, 4 and 5 examine the organization of the communities and groups' roles in manipulating citizens to protect the interests of the government and the PRI. The government and party cultivate diverse links with social, economic, religious and political groups and their leaders as cooptative and incorporative mechanisms. Chapters 6 and 7 examine economics from the perspectives of both the enterprise and the worker, demonstrating how class factors affect community economic development and worker occupation choice and mobility. The concluding chapter is followed by a lengthy methodological appendix, including Eckstein's questionnaire for residents. Much of the work gives very useful observational data that largely support the very plausible thesis that the Mexican government and the PRI manipulate the masses in the interest of dominant economic forces.

Two particularly notable features of the book, its stylistic and logical inconsistency, constantly confront and frustrate the reader. Ideological tone and writing style vary wildly from chapter to chapter. The critical leftism of chapters 1 and 8 reveal Eckstein at her best, but the turgid and uneven prose of chapters 2 and 3 betray a structuralist orientation possibly left over from her dissertation. The reader also encounters frequent logical lapses and contradictions; although few of these obscure the ultimate message, they cloud many issues and leave numerous unanswered questions. A particularly jargonistic and unwieldy section on cooptation theory appended to chapter 3 should have been deleted. Its principal insight is tautological: "Organizations may not serve as instruments of political power . . . if they lack institutionalized access to authoritative power" (pp. 106–07). Such basic mechanical problems suggest to this reviewer the extreme poverty of the editorial efforts of the publisher

The inclusion of the questionnaire raises another question: Why has Eckstein not exploited this wealth of data? She uses it in the chapter on occupational choice, but largely ignores the data elsewhere. In chapters 3 and 5 Eckstein treats class influences upon activity in and of social, economic and political organizations, yet she does not employ several items in her questionnaire that could shed substantial light upon such behavior. The interview asked numerous questions about income, occupation, education, and perceived class status that could have provided an index of class membership. One group of items measured membership and activity in organizations, and several others indicated voting, party membership, ability to contact public officials and various political beliefs and attitudes. One wonders why Eckstein overlooks such a gold mine of material. The impressionistic data the author does present go a long way toward supporting her conclusions, but by failing to include available and relevant empirical data at the individual level, the author raises further doubts: Does the material excluded fail to support her conclusions? If this material does support the thesis, why not present it or at least provide the reader with a brief explanation for its exclusion? A similar problem arises with Eckstein's conclusion that groups provide no basis for effective representation of the interests of the poor. Despite the presence in her data set of material that would permit evaluating perceived needs among community residents, demand-making activity such as contacting public officials, and attitudes regarding individual perception of government benefits, virtually none of these data surface in the analysis. Once again, Eckstein's observational data provide valuable insights and an overview of techniques of the government and party for manipulating demands. However, to evaluate the success and appropriateness of demand-making by residents seems to require the use of much of the overlooked material. But without such a requisite analysis of perceptions, demands, and reward distribution—seemingly easily within her reach—Eckstein concludes that class, hierarchical and corporate-group pressures "inhibited local poor from using those groups to which they gained access principally for their own ends" (p. 209). I remain with the impression that Eckstein often imposes her conclusions upon the analysis without permitting relevant data to speak directly to the issues.

Ultimately, Eckstein must be commended for the breadth of her undertaking and for the perceptiveness and detail with which she delineates the linkages between the communities and the formal government and party apparati. The very breadth of the work, however, also contributes to some of the volume's shortcomings: Covering so many aspects of the problem of community-state linkages may well have obviated the deeper empirical analysis needed to answer the questions raised. When Eckstein goes outside her discipline, the analysis loses focus; for example, the treatments of economic and political issues are weak in comparison to the analysis of occupational choice.

Because of its analytical flaws, self-contradictions, and uneven style, not to mention its high price, *The Poverty of Revolution* as a whole will not make good material for undergraduates. But for the mature student or scholar interested in either Latin American urban politics or political participation, this book can provide valuable insight into how the Mexican state deals with the urban lower classes, managing their participation for its own purposes.

JOHN A. BOOTH

University of Texas, San Antonio

Legislatures in Plural Societies: The Search for Cohesion in National Development. Edited by Albert F. Eldridge. (Durham, N.C.: Duke University Press, 1977. Pp. xv + 284. $12.75.)

The publication of several volumes of comparative legislative studies since 1970, and the establishment of a consortium and a journal in this field, reflect a growing concern for comparisons among the specialized or regional interests of students of legislatures and legislative behavior. *Legislatures in Plural Societies* combines this concern with a focus on the role of legislatures in promoting political integration in ethnically and culturally fragmented societies.

It is difficult to review a symposium, let alone one that covers countries as different as Afghanistan, Canada, the Indian State of Rajasthan, Lebanon, Northern Ireland and Yugoslavia. Nor is my task made easier by the assumed "common" research problem: "The problem of the scholar . . . is to determine how the experience of representational, decisional and legitimating functions (of legislatures) may affect national integration" (p. 7), since "there is no reason to suppose that there is something inherent in the legislative process that guarantees either integration or disintegration in a deeply divided society. . ." (pp. 15, 205). The six case studies in this volume amply confirm this assumption. Given the system-wide ramifications of integration processes and the fact that each study necessarily examines only the problems and strategies relevant in a given setting, it is not surprising that the focus on legislatures appears to be primarily a convenient vantage point from which to explore integration-relevant political processes.

Malcolm Jewell reviews the role of electoral systems and representation, and establishes an agenda for the study of the behavior of legislators chosen from minority groups, both in the legislature and in relations with their constituencies. More importantly, he reminds us of the need to study not merely the symbols, but also the tangible results of minority representation, i.e., whether and how legislative outputs enhance support for the system.

Allan Kornberg's and Samuel Hines' examination of the integrative role of the legislature in the years preceding and immediately following the establishment of the Dominion of Canada, is one of the more interesting case studies. Having defined integration and modernization as different facets of empirically overlapping processes, the authors ask why Canada's high level of modernization has not been matched by a corresponding level of national integration. Their content analysis of debates in the early years of the legislature is really only a first step in the study of this problem. The authors consider the decision to develop the transcontinental railroad as Parliament's single most important integrative act after 1867; the ensuing modernization, industrialization and tariff policies became concomitants of the integration of the western and maritime provinces. The evidence presented in this study indicates that problems of ethnic-religious identity represented only a minor fraction of the themes articulated in Parliament; in that period, however, those French Canadians who preferred the larger confederation to the previous Union with Ontario perceived the confederation as a means of obtaining exclusive control of their own affairs. Subsequent legislatures and governing elites did not provide many unifying symbols, and a regionally uneven distribution of resources and investments eventually resulted in uneven modernization. One now looks forward to similar analyses of Canadian legislative politics in the more recent period for which the above hypotheses are being offered.

It is ironic that Abdo I. Baaklini was able to make an almost convincing case for the merits and success of the Lebanese "National Pact" between the major religious and ethnic groups precisely at the time when the entire regime collapsed. It is true that the presence of armed Palestinian forces in Lebanon precipitated the civil war, and that the conflict had political as well as inter-religious dimensions. Yet, whatever integration may have been achieved through a system of reserved seats in the legislature and the distribution of the major executive offices among the three major groups, political integration must have been more apparent than real. Was not the reluctance to establish a strong Lebanese military force motivated by the same intergroup suspicions that eventually led to the maintenance of "private armies" and to foreign intervention?

That sociocultural conflict can determine institutional performance, or even demise, rather than be resolved by institutional engineering, is also the theme of Lenard J. Cohen's contribution on Yugoslavia and the chapter by Ian Budge and Cornelius O'Leary on Northern Ireland. Despite institutionalized legislative representation of the constituent nationalities of Yugoslavia, the management of conflict—through cross-regional bargaining over the distribution of costs and benefits of development policies—was made possible only through the intervention of the "unifying force" of the Communist party. Budge and O'Leary describe the uncompromising domination of the Unionist Protestant majority in Northern Ireland through 1973, which led to the demise of the regime. In the absence of tolerance for the Catholic minority, even a seemingly more moderate attitude in the 1960s merely strengthened the extreme militant wing of the Unionist party and made the survival of the parliamentary regime dependent on extremist support and thus on the perpetuation of the religious conflict.

Richard Sisson and Lawrence L. Shrader focus on the "vertical" integration of society by the "new men" in politics in Rajasthan. This is essentially a study of social and behavioral characteristics of legislators in the modernizing caste society of the former princely states, emphasizing the links between traditional origins, parties, cross-cutting factional coalitions and the functions of modern government. Marvin G. Weinbaum's chapter on the legislator as an intermediary between center and periphery in Afghanistan describes a paternalistic regime lacking political parties, as well as national issues capable of overriding traditional divisions and personal interests. With a legislature not expected to perform orderly legislative functions, the "legislator" is primarily an intermediary between members of the constituency and the administration, in exchange for private remuneration.

This volume is a very useful addendum to the 1970 study of *Legislatures in Developmental Perspective* edited by Kornberg and Musolf, and Kornberg's 1973 symposium on *Legislatures in Comparative Perspective.* It is more limited in scope, but presents evidence from infrequently researched areas. It is also theoretically less ambitious. This is not surprising, since, admittedly (p. x), the study displays "probably insufficient definitional or theoretical unity." One should add, however, that a lack of definitional rigidity for the concept of integration is not necessarily a shortcoming in an area of essentially exploratory research, especially when one considers the underlying assumption of the volume. I have more difficulty with hesitations to operationalize specific interpretations of integration. What are the empirical referents of integration? How can we determine the "degree" of integration that has been achieved? Are we limited to negative values on this dimension, i.e., observations of disintegration and collapse? Moreover, is the integration of a pluralist society a definitive, or a frequently challenged and changing condition? Kornberg and Hines (p. 204) suggest a quasi-operational definition of "balance" which could serve as one measure of integration: "the widely shared belief . . . [that] approximately equal weight [is given] to interests of the several major . . . groups and regions." One could thus ascertain "how widely shared" such beliefs are, and in regimes where beliefs are not easily researchable, integration could be analyzed in terms of policies and policy outcomes. Even only rough estimates of the degree of integration could provide added meaning to the comparative study of the role of actors, parties, institutions and processes in political integration, and perhaps some benchmarks for hypotheses associating levels of integration, institutionalization and modernization. With observations limited to disintegration and collapse, integration becomes synonymous with the survival of the system.

MOSHE M. CZUDNOWSKI

Northern Illinois University

Ethnic Conflict in the Western World. Edited by Milton J. Esman. (Ithaca, N.Y.: Cornell University Press, 1977. Pp. 399. $17.50.)

In its December 7, 1977 issue, the well-known French newspaper *Le Monde* carried, in its front-page "Bulletin from Abroad" column, an editorial-like essay on the "daring" and "good sense" of the Catalàns for having achieved "what the Communists call the only national front government existing actually in Europe." This was the autonomous government formed in Barcelona the previous December 5. Inside, the paper carried two articles on that regional government (p. 3). Further inside, in the cultural section, there was a brief article on the recent opening of a Center of Catalán Studies at the University of Paris-Sorbonne (p. 23).

Still, in the same issue, *Le Monde* reported a bloody incident in Malaga, Andalusia, where a riot had broken out during the funeral of a regional labor leader who had been killed in the course of a demonstration in favor of Andalusian autonomy (p. 3). And lest one should think that such problems and issues are peculiar to Spain alone, the *Los Angeles Times* around the same period carried an article about the trials and tribulations of the Sorbs, a small Slavic minority in Germany.

What all these pieces have in common is that they focus on ethnic or ethnoregional identity, problems, and conflicts in Western European countries. Ethnicity has become indeed a major issue of our times. But the point is not merely that ethnicity per se has become salient. It is rather that ethnicity-in-the-Western-world has become salient as a phenomenon and as an area studied by social scientists and historians. This book, which results from a conference on ethnic pluralism and conflict in contemporary Western Europe and Canada, held at Cornell University in May 1975, purports to examine the re-emergence to political significance of ethnicity and ethnic mobilization in those "industrialized and affluent societies" (p. 11).

The book displays the usual problems of the essay collection format: disparities in interest and orientation, uneven quality, and stylistic differences. In this particular case, there is no theoretical framework that links the contributions together, no unifying paradigm. They are united essentially by the concept of ethnic conflict with all its nuances and variations, insofar as it applies to those Western countries. It is also clear from the outset that the essays deal only with ethnic groups that have an established territorial base, an ancestral homeland, as it were. That, according to Milton Esman, excludes blacks in the United States and migrant workers in Europe. It does seem, however, to include the French Canadians to whom two papers are devoted. Aside from this minimal thematic unity, the articles range widely in qualitative terms: they go from the descriptive/historical to the theoretical/analytical, from the ordinary to the systematic. It must be added, however, that consociationalism is a recurring theme in the essays and is a significant concept in at least six of them, including one which specifically asks, "Does the Consociational Theory Really Hold for Switzerland?" Thus, consocialism could in a sense be viewed as a partial unifying theme of the book.

Apart from its introduction, the book is divided into three greatly uneven parts. Part 1 focuses on historical and theoretical explorations; part 2 presents eleven case studies, two each on France and Canada; part 3 consists of a summarizing essay by the editor. While that last chapter is a rather repetitive exercise—especially since it is really an enlarged and extended version of the introduction, a frequent problem in this kind of book—it constitutes a very valuable statement of propositions, hypotheses, and findings on the causes, tactics, management, international implications, and outcomes of ethnoregional mobilization in Western industrial societies. It effectively pulls together the strands spun by the various authors.

In part 1, Walter Connor and Arend Lijphart criticize the hitherto conventional wisdom among students of the West, which consisted in down-playing both ethnonationalism and the nationalism of established states in favor of supranational integration, and they attempt to explain the current resurgence of ethnoregional movements. In warning against an excessive reliance on materialistic interpretation, however, Connor shows an excessive preference for a suggestivistic interpretation of ethnicity. Both he and Lijphart fail to consider seriously the class dimension, although the latter discusses "the 'horizontalization' of vertical ethnic groups" as a factor of mobilization. The class element is implicit, however, in Lawrence Scheinmann's pessimistic analysis of the relationship between ethnoregionalism and European integration, which he sees as an instrument for maintaining the capitalist system and existing power relationships.

Part 2 contains articles on the Basques, on Belgium, France, Canada, Northern Ireland, Scotland, South Tyrol, Switzerland, and Yugoslavia. It is noteworthy that William R. Beer tries to discover, through a leadership survey, the social class makeup of ethnic activists in France. What he calls classes, however, are really occupational groups. On this basis, he finds that it is the career frustrations of professional and white-collar workers, especially teachers, which explains the rise of ethnoregional discontent.

Also of interest is Jacques Brazeau's and Edouard Cloutier's chronicle of the breakdown of consociationalism in Quebec. They do not, however, consider the general implications for other plural societies. This challenge is taken up by Jürg Steiner and Jeffrey Obler who, in studying the Swiss case, cast doubt on the long-range consequences of consociationalism and raise the question as to whether and when to shift to a majoritarian/competitive model. Susan Bridge, in discussing Yugoslavia, also raises the problem of the appropriateness and timeliness of consociationalism. Finally, in the context of South Tyrol, Peter Katzenstein sees some interesting correlations between occupational structures and responsiveness to ethnic appeals. However, these correlations need some clarification, especially in light of Beer's findings in France.

The book as a whole is a very good introduction to the study of ethnoregional cooperation and conflict in Western states.

PIERRE-MICHEL FONTAINE

University of California, Los Angeles

The Military Coup d'Etat as a Political Process: Ecuador, 1948–1966. By John Samuel Fitch. (Baltimore: Johns Hopkins University Press, 1977. Pp. xv + 244. $15.00.)

As everyone knows, the military coup is one of the most characteristic features of Latin American politics. Over the last 15 years, it has passed from the realm of folklore to that of serious study, but a great deal of our new knowledge, as might perhaps be expected, has served not to answer fundamental questions but only to complicate them. Scholarly and journalistic accounts of individual coups have highlighted a range of causal explanations, which have been partially and rather clumsily synthesized in several competing theoretical models.

John Fitch has chosen Ecuador, which lags not far behind Bolivia, the all-time champion in the frequency of coups d'état, as the locus for a Herculean attempt to construct a model of the military coup, incorporating all of the relevant causal factors, determining the modes of interaction among them, and assigning relative causal weights. Against all odds, he has achieved a high degree of success, producing a classic work which must be the starting point of future studies of the military coup. Fitch's success is not total; in the nature of things, the problem is too complex to permit an adequate specification of all the relevant variables, some questions were not explored as fully as one might wish, and some of the conceptual organization seems arbitrary. But it would be churlish not to acknowledge the magnitude of Fitch's achievement.

The book deals in detail with four coup attempts between 1948 and 1966, and in a final chapter discusses coup activity during the last ten years. One of the book's strengths lies in studying the coup process over time since, as Fitch makes clear, different causal factors were present, and the same factors had different weights, during different coups. It thus becomes clear how different schools of thought have arisen, based on what was believed to be the same set of data, but were really quite different sets.

In general, Fitch's point of view is that of the "institutional-interest" school of explanation: "The key underlying concern for the Ecuadorian military has been the protection of the institutional interests of the armed forces" (p. 126). This is certainly true at the level of analysis employed in the book; however, general theories which hold that, regardless of the subjective motivations of the military in staging a coup, objectively they are serving the interests of the successful functioning of the system, or of a specific class, are not refuted by the book's conclusions. Fitch shows the importance, as a determinant in the decision to overthrow the incumbent government, of the military's attitude toward its constitutionality, and their perceptions of whether a crisis situation exists, both of which lend support to the conception of the military coup as a mechanism by which the system maintains itself; he also stresses the importance the military assigns to "public opinion," which turns out on examination, however, to be principally the line taken by the major newspaper, *El Comercio,* which, as he recognizes, represents primarily the view of the *sierra* middle classes.

The decision to stage a coup is made in response to the military's perception of the extent of the political crisis, critical components in which are perceptions as to the government's constitutionality, its treatment of the armed forces, its attitude toward Communism, the level of public disorders, the state of public opinion, the perceived need for structural reform, and the personal ties or antagonisms of military officers. The relative weight of each of these factors in military perceptions is quite different from coup to coup. Moreover, the attitudes of individuals vary with their conceptions of the proper role of military officers, which Fitch categorizes as constitutionalist, professionalist, arbiter, or developmentalist (pp. 129–31). The frequency of the different role definitions has changed with the rise of professionalism following the founding of the military academy in 1925 and the experience of the military defeat by Peru in 1941, and then with the stress on development which became generally accepted in the 1960s.

In this short space it is not possible to do justice to the richness of Fitch's insights and conclusions, or to his grasp of the material. The fullness of his treatment of the subject matter, based both on a solid knowledge of Ecuadorean history and on in-depth interviews with a hundred significant participants in the events he is dealing with, make this book as worthwhile for the specialist on Ecuador as for the student of the military coup.

MARTIN C. NEEDLER

University of New Mexico

Rural Catalonia under the Franco Regime: The Fate of Regional Culture since the Spanish Civil War. By Edward C. Hansen. (New York: Cambridge University Press, 1977. Pp. x + 182. $14.95.)

The Catalán provindes of northeastern Spain have struggled to maintain some degree of autonomy from the predominantly Castilian Spanish state since the fifteenth century. Regional cultural traditions and the Catalán language (which is closer to French than to Spanish) have persisted well into the modern era. Catalans, moreover, have waged war against Spanish armies on at least five separate occasions, in efforts to maintain or restore regional political institutions and to preserve the Catalán cultural heritage. The most recent of these clashes was the civil war of 1936–1939. Spanish Nationalist forces under General Franco emerged victorious from that conflict, subordinated the Catalán provinces to a rigidly centralized state administration, and attempted to repress the Catalán regional culture and language. What has been the fate of Catalán regionalism after nearly four decades of Franquist authoritarian rule? *Rural Catalonia under the Franco Regime* is an uneven and only partly satisfactory attempt to answer that question.

Edward Hansen is an anthropologist whose profession and training should have prepared him well for a study of this kind. Indeed, the middle chapters of the book, which describe changes in certain aspects of Catalán culture, are quite well done. Hansen closely examines three sets of institutions which are central to that regional culture: (1) the *rabassa morta* system of sharecropping; (2) the *hereu-pubilla* marriage-inheritance system; and (3) various cultural-recreational associations. Under the *rabassa morta* system, the sharecropper (*rabassaire*) pays the landowner one-third of his annual grape production, but as is not the case in other sharecropping systems, is given a reasonable guarantee of tenure on the land over a period equivalent to the life-span of the average grapevine (about 50 years). The *hereu-pubilla* marraige-inheritance system is a peculiar Catalán version of primogeniture, under which the eldest male or female heir inherits all fixed properties, but, in return, shares the more liquid assets with the other offspring and assumes some financial responsibility for underwriting their various business ventures. Both the *rabassa morta* and the *hereu-pubilla* systems were central elements in the rural Catalán economy of the nineteenth and early twentieth centuries; both are prone to considerable ambiguity, conflict and litigation, however, and both are gradually disappearing (being replaced by wage labor and partitive inheritance, respectively). Similarly, cultural-recreational associations, the principal repositories of regional culture, appear to be on the decline, especially among younger Catalans. Taking the place of these associations is the ubiquitous culture of the Spanish bar. Hansen's anthropological skills pay off most handsomely in his exploration of bar culture: his extensive observations of personal interactions in the bars of a Catalán village provide valuable insights into the nature of clientelism, social status and *caciquismo* in this Mediterranean culture, as well as the apoliticism and apathy so characteristic of Spanish society in the final years of the Franquist regime. The descriptive chapters of this book are by far its best.

Unfortunately, the author has a more ambitious goal in mind. Hansen rejects the parsimonious notion that these social changes are "predictable outcomes of modernization" (p. 154); instead, he attempts to interpret these social transformations as products of the spread of "dependency capitalism" throughout the Mediterranean. The author posits the rather dubious notion that Catalán regionalism (which he does not clearly define) was almost exclusively an upper middle-class phenomenon, then sets forth an economic reductionist explanation of its alleged decline: Catalán regionalism has waned because the Catalán upper bourgeoisie has been subordinated to "dependent capitalism, of which the Franco regime is a modern expression" (p. 155). This interpretation of the "collapse" (sic) of Catalán regionalism can only be supported through a highly selective reading of Spanish history: Hansen's historical chapter on "The Growth of Modern Catalonian Regionalism" ends with the 1920s dictatorship of Primo de Rivera and totally ignores both the Second Republic and the Franquist era! Thus the author is freed of the necessity of dealing with historical developments which would not fit within his theoretical framework. He does not acknowledge, for example, that by the 1930s the conservative, business-dominated Lliga had been superseded by a leftist regional party, the Esquerra, which drew its support mainly from the lower middle classes and *rabassaires*. Similarly, he sidesteps the 1940–1957 period, during which the Spanish state (far from furthering "dependency capitalism") embraced autarchic economic policies, which greatly impeded foreign trade, rigidly restricted the importation of foreign capital and encouraged the development of domestic industry through subsidies from various state and parastate agencies. Space does not permit rebuttals of other omissions and misconceptions–errors which seriously undermine the credibility of several chapters of this book.

Given his anthropological expertise but (judging from his strikingly short bibliography) relative lack of familiarity with the past 50 years of Spanish history, Hansen would have done better to have explored more thoroughly the social and cultural facets of regionalism. In his efforts to advance his economic interpretation of micronationalism, Hansen overlooks two such factors which I regard as obvious and highly important: at no point does he tell us how many inhabitants of his village still speak of read Catalán, nor does he deal in any way with the transmission of cultural values through the education system or the family.

RICHARD GUNTHER

Ohio State University

Soviet Political Elites: The Case of Tiraspol. By Ronald J. Hill. (New York: St. Martin's Press, 1977. Pp. vii + 226. $16.95.)

Hill's elite analysis of Tiraspol, a typical small Soviet city in the Moldavian republic, adds to our knowledge of local Soviet politics accumulated through an increasing number of studies published by Western sovietologists over the last decade. These scholars reflect a growing sensitivity to the importance of the local level as the only meaningful experiential realm of politics for most Soviet citizens and as the pivotal locus from which long-term political change in the USSR may emerge. Both the strengths and the weaknesses of the Soviet political system are most clearly reducible to understandable human terms in the interplay of factors that condition local political life in the USSR. Long despairing of their relevance to the mainstream of Western political science, Western sovietologists have also found their studies of Soviet local government, based on frameworks and methodologies adopted form the subfield of Western local politics, an important analytical bridge for their contribution to political science.

Hill examines the patterns of elite recruitment and composition in Tiraspol from 1950 through 1967, and he bases his elite profile on those 1,094 individuals who have held elected positions in the Tiraspol urban government council and party committee over these 18 years. Through bivariate aggregate analysis of attrition and tenure within this elite group, participatory rates in council sessions and party plenums, membership overlap between both institutions, and the composition of executive state leaders and party bureau members, Hill distinguishes between a uniform clustering of "strong" and "weak" characteristics associated with greater and less political influence in Tiraspol since 1950. Among characteristics associated with greater influence are male sex, membership in the Communist party, "mature age" over 40, and high-status occupations like party-state *apparatchiki* or industrial managers. Confirming conventional views of the Soviet system in Western studies, Hill provides solid evidence that Tiraspol is a prototypical Soviet locale in which power is controlled by older male *apparatchiki,* insensitive to local problems and assigned to leadership positions from outside the city as a career post. Even formal debates of issues in Tiraspol are dominated by an interchangeable corps of like-minded party "activists," who can be counted on to make the same predictable comments at state or party forums.

In contrast, women, those under 40, nonparty deputies, and those in low-status occupations like the consumer service sector may be elected in large numbers to the urban council or even to the party committee; but, as token symbols of Soviet democracy, they serve for one brief term, are rarely elected to leadership positions in the institutions, and are resigned to giving passive mute witness during the tedious council sessions and party plenums dutifully convened and recorded to prove the effectiveness of local institutions to central officials. Hill's findings lead him to question whether this rigid authoritarian dimension of Soviet cities like Tiraspol will not effectively undermine the intent of recent changes in the USSR, by which localities have been granted increased political autonomy and taxing authority supposedly to enhance local community involvement in policy making.

Hill's study has both virtues and shortcomings, although most of the latter are inherent in all studies of Soviet local government. Perhaps we should praise Hill most for having successfully completed his research in the first place. Hill compiled his elite data through a meticulous reading of 18 years of the Tiraspol urban newspaper while he was an exchange student at the Moldavian Academy of Sciences in 1967–68. For those of us who know that no other Western social scientist has been allowed to conduct research in Moldavia and that Western scholars confront immense difficulties in gaining access to the local press in the USSR, Hill's dual accomplishment alone marks the study as an invaluable data source unlikely to be replicated in the near future. At the same time, as Hill himself admits, the very nature of his primary source, the local urban newspaper, severely limited the scope of his inquiry and clouded the validity of his conclusions. Trying to correct for the limitations of local newspaper reporting, Hill very early in his research had to give up on interviews with local Tiraspol officials, who persisted in falling back on the hackneyed responses of Soviet officialdom. Like many other Western sovietologists, Hill has been forced to design his research to fit the available data and to extrapolate the maximum from a very incomplete and biased source. As Tiraspol is a community outside the Russian republic, Hill also should have considered the extent to which ethnic factors influenced elite recruitment within his discussion of the urban setting. Overall, however, within these limitations, Hill has written a fair and carefully researched monograph on Soviet local elites that should prove informative both to Western sovietologists and to students of comparative local politics.

JOEL C. MOSES

Iowa State University

Arab Politics: The Search for Legitimacy. By Michael C. Hudson. (New Haven: Yale University Press, 1977. Pp. xi + 434. $22.50.)

Two types of political literature, mostly unsystematic and generally poor, have been available to students of the politics of the Arab region. The first type is of the "Governments and Politics of the Middle East" variety in which Arab politics appear as a subgroup; the second type discusses more specifically the politics of one or more Arab countries. Rarely is either type of literature guided by theory and more frequently it suffers from the absence of solid data. Hudson has overcome both problems: he has dealt, within a comparative framework, with the totality of Arab politics; he freed Arab politics from its Middle East stranglehold; and his work is guided by a theory, replete with reasonable and diverse data, and informed by history.

In the not too distant past, Arabs identified with a political system and agreed to be governed in accordance with rules and norms clearly or ambiguously derived from the Islamic belief system. The legitimacy of the system as well as that of the authority was neither questioned nor challenged. While the criteria of effective citizenship was clearly demarcated, popular participation was neither enjoined nor expected. The entire system came under varied internal as well as external, ideological and physical attacks. Today's Arabs are citizens of 22 sovereign states—at the time Hudson was completing his book, these were only 18—who define themselves as Arab and in a few cases call themselves Arab and Islamic; some are monarchies, while others are modernizing patriarchies or revolutionary republics. All of them face formidable tasks: they must meet the aspirations of their citizens with varied economic resources at their disposal and a relatively underdeveloped administrative system, while trying to create an effective and viable political structure and frontiers of the political community. That the task is far from being successfully, evenly or comprehensively accomplished is shown by the highly volatile nature of Arab politics. From one end of the Arab world to another, Arabs are constantly challenging the legitimacy of their political system; quite frequently the challenge is violent and often it is guided by a competing ideology. Sometimes governments are able to cope with the challenges: they may resort to traditional legitimating norms, charismatic appeals, or any combination thereof. Often they may meet the challenge by control and coercion, which gives them some respite.

Hudson's basic argument is that turmoil in Arab politics is to be explained in terms of the extent to which the Arab states and their regimes have acquired the necessary legitimacy to stay in power. Questions of identity, authority and social change are at the root of the problem. Citizens of each Arab state tend to retain older loyalties while adding new ones. Is there a basic conflict between being an Arab and a Syrian? What concrete political consequence follows from holding both identities? While there is no clear answer to this and similar questions, it is important to note that there are viable political groups, some of whom hold political power in one or more of the Arab states, who adhere to an ideology that requires political integration at the Arab level. Other groups defend the existing Arab state system and view the attempts of their Pan-Arab rivals with grave suspicions. Hudson discerns increasing acceptance of the existing system without abandoning some core issues such as the resolution of the Palestine question. Whatever the ultimate answer may turn out to be, the question of Arab identity, which is entangled with language, culture, religion and political aspirations, will continue to plague Arab politics.

The Arab states are now a reality; so are the multiple Arab governments. Who governs and in accordance with what norms, values and how well, is Hudson's second question. Here too tradition and modernity intersect. Previous governments ruled in accordance with norms derived from a basically traditional structure enjoined by Islam. Yet the traditional structure has been seriously and irrevocably undermined and Islam as a system of political beliefs—not as a religion—has been significantly transformed. While contemporary Arab governments may invoke one or the other to justify their authority, their citizens are concerned with the performance of specific secular and modern norms of governing. The discrepancy between the actual and the ideal underlies what Hudson calls the "crisis of authority." To achieve legitimacy, Arab governments have to institutionalize structures of participation which are conspicuous by their absence throughout the Arab world. Increasingly, Arab regimes, while able to meet the secular needs of their constituents, resort to coercion and control to maintain authority.

The third dimension of the legitimacy question is what Hudson terms the "equality." While Arab regimes have carried out important and in some cases revolutionary programs in education, health, welfare, and they strive for a higher standard of living and the reduction of economic and social inequalities, they have failed miserably in their effort to bring about a modicum of political equality which would mean popular self-government. The detailed "case studies" of each of the 18 Arab states, grouped analytically under three categories—the modernizing monarchies of Jordan, Morocco and the Arabian Peninsula; the Republics of the Pan-Arab core of Egypt, Syria, Iraq, Lebanon and the Palestinian movement; and the republics of the periphery of Sudan, the two Yemens, Libya, Tunisia, and Algeria—do portray in some detail the problems and performance of each of these regimes in terms of "equality." While in general the Arab states have done rather well considering the domestic and external constraints, without exception they have failed even when they seriously tried to institutionalize political equality. Recent events in the Arab world such as the total breakdown of Lebanon and the severe crisis of Tunisia which may foreshadow another breakdown may remind the governments of the Arab states that administration is no substitute for political participation.

All students of Arab politics are in Hudson's debt; he has written probably the most significant and persuasive book on this subject, amply documented. Equally important, Arab governments and their opponents have much to learn from this volume. Is it too much to expect its early appearance in paperback and in an Arabic rendition?

IBRAHIM ABU-LUGHOD

Northwestern University

The Silent Revolution: Changing Values and Political Styles Among Western Publics. By Ronald Inglehart. (Princeton, N.J.: Princeton University Press, 1977. Pp. xiii + 482. $30.00, cloth; $9.95, paper.)

Ronald Inglehart finds evidence of a potentially revolutionary integenerational shift in political values in the West deriving from the post-World War II era of peace and prosperity. In marked contrast to their elders, who grew up under the harsh conditions of economic crisis and physical insecurity stemming from two world wars, young Europeans in the postwar period spent their formative years in good times and their political values reflect these favorable conditions. In accordance with Abraham Maslow's value hierarchy, because young Europeans have less need to be preoccupied with material sustenance and physical safety, their priorities have shifted upward toward meeting nonmaterial needs for belonging and self-actualization.

Post-materialists (as Inglehart terms them) differ from materialists in many ways: they are less parochial in outlook, identifying with their nation-state and the European Community, rather than with their locality or region; they are more likely to be affluent, politically informed, educated, and skillful. These differences have important political implications. Rather than being content with passive and traditional participation, e.g., voting or membership in hierarchical organizations like the political party, post-materialists seek active participation in nonhierarchical, issue-oriented movements, and they seek to participate in making decisions rather than merely choosing leaders. Post-materialist demands made by skillful, committed, middle-class youths are more absolutist and less negotiable than the quantitative economic demands of workers. Post-industrial cleavages rooted in post-materialism are joining class cleavages and pre-industrial cleavages. Governments geared to meeting material demands of an industrial society through stimulating economic growth are ill-equipped to meet the new qualitative demands, especially because material and post-material demands may conflict.

The generational cleavage between materialists and post-materialists is strongly influenced by the national setting, in particular, the extent of political-economic change. At one extreme among Western nations, Germany achieved a dramatic transformation from wartime devastation and poverty to prosperity and stability. At the other extreme, England did not start so low or end so high. Inglehart finds that intergenerational contrasts are substantially greater in Germany, indirect confirmation that the shift in values reflects intergenerational rather than life-cycle changes.

The Silent Revolution is an ambitious cross-national survey of value change, employing a host of intricate techniques and uncovering a wealth of interesting relationships. Although it may seem that enormous energy is devoted to pinpointing an obvious overall trend, the truth frequently does seem obvious—in retrospect. More serious defects, however, are that the book barely explores the content of the silent revolution; it extrapolates from a tendency originating in an expansionary epoch and ignores the impact of what will likely be a stagnant future; and, by exaggerating the impact of values on political conflict and change; it misconstrues the character of political power.

A first question involves the character of post-materialist values. Since, in a factor analysis of value choices, the most "post-materialist" item turned out to be democratizing work, one might question the validity of the concept of post-materialism. Post-materialism may be concerned with other aspects of material life than demands involving wages and prices, but this is quite a different matter from arguing that it is unrelated to material issues and class cleavages. In fact, Inglehart's post-materialists seem to be challenging the deskilling and hierarchy of intellectual labor analyzed by Harry Braverman.

A further question about the validity and utility of the material and post-material distinction is that the electorate itself apparently fails to see the two as very distinct. This cannot be judged directly since not one of the more than 30,000 respondents in the 11-nation sample is ever quoted directly. On the basis of their responses to four questions on values, over half the respondents were classified neither as materialists nor as post-materialists, but were placed in a mixed category. Moreover, the ranks of the post-materialists are especially meager: only one-tenth of the entire sample is post-materialist and, among all eleven nations, only in Belgium does the proportion of post-materialists exceed one-fifth of the most heavily post-materialist cohort. The post-materialists may be an avant garde whose significance is disproportionate to their size, but extreme caution seems necessary in advancing claims about their impact. Yet, to cite one example, Inglehart emphasizes the post-mateialist character of the May 1968 movement in France and the conservative swing of the working class in the June 1968 elections, but he hardly mentions the critically important role of the working class in the nationwide strike movement itself. Instead, he concludes, in part on the basis of the May movement, that "the advocacy of social change [in the late 1960s] tended to have a middle-class rather than a working-class base. . ." (p. 285).

Second, Inglehart argues that the impact of post-materialism will accelerate as successive cohorts of young post-materialists replace elderly materialists. However, for such a replacement to occur, the next quarter-century would probably need to resemble the last one. (He himself notes [pp. 106, 114–15] that the shift toward post-materialism would be reversed by major economic difficulties.) It would require a Dr. Pangloss to believe that a repetition of the postwar boom lies ahead. Despite the fact that Inglehart generally accepts the Club of Rome's dire analysis (although he seeks a way to avoid their prognostications), he continues to view the future from the perspective of the unprecedented postwar period. Judging from a survey carried out in 1976, whose results he summarizes, the young growing up in the 1970s already differ from their post-materialist elders who grew up in the 1950s and 1960s; any trend toward post-materialism has doubtless slowed even farther since then.

Third, this raises the general issue of the political relevance of the value changes he discerns. Aside from the issue of possible trends in the number of post-materialists, I would question Inglehart's tendency, as in the following passage, to infer structural change from changes in individual values and skills (and this despite his argument that the general causal flow was in the opposite direction regarding the origin of post-materialist values and his repeated assertion of the importance of political structures intervening between values and politics): "Special skills are a prerequisite for playing an effective role in the politics of an extensive political community embracing millions of citizens. Consequently, rising levels of political skills should enable Western publics to play a more meaningful role in the decision-making process" (p. 368).

It is one thing to explore changes in mass electorates, quite another to draw conclusions from such changes about the overall distribution of political power in the society. The word "power" does not appear once in the whole book, to my knowledge; a frequent surrogate is "participation." The claim that rising numbers of post-materialists will signify greater popular participation in political decision making is triply questionable: first, political skill is measured (for reasons that are unexplained) by frequency of political discussions. Second, why are special political skills a prerequisite for playing an effective political role? Courage, solidarity, and motivation might be of equal or greater importance. Third, even if political skill were a necessary condition for effective participation, it is not a sufficient condition. There is an implicit equation here of skill and power: it is argued that a more skillful electorate will "narrow the gap between elites and masses" (p. 296). But the power of political authorities does not derive primarily from their superior forensic ability or other political skills but rather from their control of institutional and material resources.

That there is a contrast in values between the prewar and immediate postwar European generations is now solidly established, thanks to Inglehart. But, as he makes clear at the outset, the structural characteristics of the political economy remain fundamental. Given the deepening crisis of advanced capitalism, the silent revolution is probably being replaced by Thermidor, and the conflict between generations or between materialists and post-materialists by renewed conflict between classes.

Mark Kesselman

Columbia University

How Long Will South Africa Survive? By R. W. Johnson. (New York: Oxford University Press, 1977. Pp. v + 327. $15.95, cloth; $8.95, paper.)

R. W. Johnson has chosen for the provocative title of his book—*How Long Will South Africa Survive*—a question frequently asked these days when internal and external pressure on the republic are accelerating. The answer he seems to give in his concluding chapter is that with adequate ruthlessness in dealing with black protest, and continued support from the West, and in particular from the United States, South Africa, presumably still with its white regime, might well last into the next century. Maybe so, particularly if American, and other Western investment and loans continue. But if one examines the book rigorously, one will find all too little in the earlier chapters, which detail successive crises, to support this conclusion. What is disturbing about this intriguing book is the gap between its massive collection of useful material and the conclusions drawn from them. The reader may well be dazzled by its fast-moving prose and the material the book includes, and overlook logical flaws in its conclusions.

This warning is not intended, however, to undercut the value of a considerable part of Johnson's material. In his first chapter, for instance, he amply documents the fact that ruthless repression of black efforts to create change, coupled with soaring levels of foreign investment, enabled the South African regime to achieve what he calls "the economic miracle of the 1960s" (p. 31) which blotted out the impact of the world-wide condemnation of the massacre of unarmed Africans at Sharpeville in March 1960. In the sixties, as he points out, South Africa broke free from its almost complete dependence on Great Britain and the United States for imports and exports to expand its trading relations to include the EEC, notably West Germany and France, and also Japan. From all these sources, South Africa was henceforth able to secure the advanced technology that its own small market was unable to generate. No less significant, particularly in the light of the arms embargo that most United Nations countries, but not France, imposed at least partially from 1963 on, was that France developed a political and strategic connection with South Africa that made it the republic's chief arms supplier as increasing internal and external pressures led South Africa to undertake a massive arms buildup.

What was no less a significant feature of the 1960s that extended into the 1970s was the massive increase in state-backed and state-owned enterprises and the intensification of the role of the state both in eliciting and in directing capital inflows. This enlarged, and indeed dominant state sector, whose reality has been largely concealed by the international image of capitalist South Africa, greatly strengthened the Nationalist government and the Afrikaner business community to their mutual advantage.

Yet, as Johnson also points out, the acquisition of a mature industrial economy forced South Africa to expand its markets. Its natural outlet was Africa, but newly independent African countries were of no mind to accept willingly an economic dependence on South Africa whose apartheid represented all they most despised. By 1964, the year before UDI, 85 percent of South African exports to Africa were going to Rhodesia and Mozambique; by the early 1970s only 2–3 percent of its exports went to black Africa. Rearming heavily placed the South African balance of payments in jeopardy. The answer, says Johnson, lay in gold, of which South Africa produces three-quarters of the world's supply. That gold is an important source of South Africa's wealth and participation in the world economy is beyond doubt. It is when Johnson describes "South Africa's salvation: the war over gold" (p. 67 ff.) that one becomes skeptical. When the price of gold is described as a political weapon used first by France and South Africa, and subsequently by Simon and Kissinger, to exert pressure on Vorster to persuade Ian Smith to agree to majority rule for Rhodesia, one's skepticism becomes still stronger.

Much of the latter part of Johnson's book deals with international politics, the increasing rivalry of the United States and the Soviet Union in Africa and particularly in Angola. Admitting that the Cubans did not send troops to Angola until after the South Africans had invaded from Namibia, Johnson accepts the latter's assertion that it did so because of American encouragement, a point consistently denied by both the Ford and Carter administrations. Only briefly, and with few of the telling details and little understanding of his descriptions in the earlier sections of his book, does Johnson deal with the most important factor in the future of South Africa: the growing consolidation of the black front. Like so many others, he underestimates its long-term potential for forcing change.

How Long Will South Africa Survive will have and should have an inevitable popularity. Its racy, irreverent style is often captivating and its portrayal of an amoral international *realpolitik* dramatic. It is likely to be difficult to persuade readers to go back to check their first impressions, but nothing is more necessary. The book can provide a vivid introduction to the issues involved in the complex southern African scene but it is far from being the last word that should be said about them.

GWENDOLEN M. CARTER

Indiana University

The Russian Revolution: A Study in Mass Mobilization. By John L. H. Keep. (New York: W.W. Norton, 1976. Pp. xvii + 614. $19.50.)

The Bolsheviks Come to Power: The Revolution of 1917 in Petrograd. By Alexander Rabinowitch. (New York: W.W. Norton, 1976. Pp. xxxiii + 393. $14.95.)

Over the past 60 years, hundreds of books have been devoted to describing and explaining Russia's 1917 revolution and the complex process by which a hitherto small and obscure socialist sect, the Bolsheviks, rose to become rulers of the world's largest country, decisively altering the course of world history. Given the sheer quantity of previous scholarly outpourings on the subject of 1917, we are entitled to skepticism about the utility of adding yet another pair of weighty tomes to the list.

Happily, these two volumes represent important new contributions to the literature. Both authors are solidly grounded in the traditional historiography of the period (Western and Soviet), and both make judicious use of the valuable materials that became accessible during the Khrushchev era. Their books devote unusually sustained attention to the activities and attitudes of lower-level participants in the revolutionary process and both insist on the fundamental importance of focusing more explicitly on the interaction of elites with mass publics if we are to arrive at an adequate account of the revolutionary phenomenon.

The book by Alexander Rabinowitch is the more geographically and temporally circumscribed of the two, confining itself to political developments in Petrograd from the abortive July uprising to the capture of the Winter Palace and the formal legitimation of the Bolshevik fait accompli by the second All-Russian Congress of Soviets. The interpretation that emerges in Rabinowitch's careful narrative is provocatively different from the conventional wisdom in several respects. Both Soviet and Western historians have generally laid great stress on Lenin's tactical brilliance and understanding of the revolutionary temper of the masses, on his successful one-man battle to overcome the confused and wavering elements in the party leadership, and on the decisive advantage to the Bolsheviks of possessing "the organizational weapon"—that is, by the centralized, hierarchical and disciplined character of a party of professional revolutionaries, which made it possible for the Bolshevik leadership to implement their zig-zag course to insurrection quickly and with minimal internal back-talk.

Rabinowitch views the events leading up to the capture of power quite differently. He emphasizes how Lenin's isolation after going into hiding to escape arrest in July caused him to lose touch with the realities of the political situation in Petrograd, and, most crucially, to underestimate the attachment of even the more radical elements among the workers and soldiers to the soviets as the principal repositories of revolutionary legitimacy. While Rabinowitch acknowledges that the Bolsheviks were more united in their actions than their major political rivals, he emphasizes that this unity *and* its alleged benefits have been greatly exaggerated. Not only were there top leaders in the Bolshevik Central Committee, such as Kamenev, who "consistently rejected almost all of Lenin's fundamental theoretical and strategic assumptions" (p. xx), but also such leaders were remarkably successful in overriding him in shaping the party's most important politics throughout this entire period—to the party's great profit. The "Leninist" character of the party was compromised even further: the Central Committee itself was often unable to control the major regional party organizations outside the capital, and even within the city of Petrograd the local party committee and the quasi-autonomous military organization did pretty much as they thought expedient and did not hesitate to defend their prerogatives.

Horrifying as such a situation must have been to Lenin, Rabinowitch suggests, it was not only inevitable (given the exponential expansion of the party's ranks in this period and the difficulties of rapid communication)—it was also, on balance, a positive development. The party's "internally relatively democratic, tolerant, and decentralized structure and methods of operation, as well as its essentially open and mass character" (p. 311) left it better able to monitor rapid shifts in public opinion and to adjust local tactics accordingly, as Rabinowitch demonstrates in considerable detail. He concludes that "the relative flexibility of the Party, as well as its responsiveness to the prevailing mass mood, had at least as much to do with the ultimate Bolshevik victory as did revolutionary discipline, organizational unity, or obedience to Lenin" (p. xxi).

John L. H. Keep paints on a much broader canvas than Rabinowitch, but many of his concerns are similar. As his subtitle suggests, Keep is concerned above all with the study of the "lower depths" of Russian society in 1917 and with the role of the mass organizations which mobilized them to political consciousness and activity—principally the soviets of workers' and soldiers' deputies in the towns and cities and their peasant counterparts in rural areas as well as other self-constituted organizations such as the trade unions, factory committees, and militia bands. While these organs were for the most part spontaneous creations of local communities whose very survival was threatened by the economic and political chaos of 1917, Keep argues that "more than any other factor, it was the Bolsheviks' ability to capture the energies contained in the most important of these bodies that brought them victory in the struggle for power" (p. x).

While Keep by no means ignores the development of mass organizations in Petrograd and Moscow, what makes his book a major contribution is its close attention to the more under-studied arenas of political struggle: the provincial cities and towns and the rural areas of European Russia, where the revolution had its own dynamics, often quite different ones from those of the metropolis. Keep is best at analyzing the impact of the wartime economy on the mobilization of town and countryside and delineating the different impact of the revolution on regions and villages with differing social and economic structures. His chapter on the peasant and the market and the spread of agrarian violence will surely become classics and should stimulate much controversy and future research.

An interesting point that emerges from both books is the often decisive importance of the revolutionary soldiery, not only in Petrograd, but also in the provinces, where thousands of soldiers were dispatched as agitators or returned home for their own reasons and assumed leadership roles. That the "men in gray overcoats" were no longer simply peasants in uniform becomes readily apparent, yet the process by which they were politically mobilized and psychologically transformed in the chaos of war and revolution still remain largely a mystery. A careful and detailed study of these uniformed revolutionaries is perhaps too much to ask of Keep and Rabinowitch, since their books already break much new ground, but clearly our understanding of the events of 1917 will remain seriously incomplete until someone undertakes this task. It is to be hoped that such a study is not far off, since evidently important documentation on the subject has begun to become available, at least to Soviet scholars.

PAUL M. JOHNSON

Yale University

World Perspectives in the Sociology of the Military. Edited by George A. Kourvetaris and Betty A. Dobratz. (New Brunswick, N.J.: Transaction Books, 1977. Pp. xi + 294. $14.95.)

Military sociology, the editors of this book argue, is an "underdeveloped subfield" of sociology. One of the reasons for this situation is that "sociology of the military has been dominated by a few individuals and groups, [and] it has [therefore] been difficult for younger sociologists with new or differing perspectives to develop their interests and gain acceptance" (p. 32).

This is an important viewpoint, for it helps to explain why, in 1973, the senior editor of this book founded a new publication, *The Journal of Political and Military Sociology*, the first academic journal committed primarily to publishing scholarly articles about the military. Until that time the only outlet for such studies was the series of anthologies published by Sage and sponsored by the Inter-University Seminar on the Armed Forces and Society (IUS). The seminar was founded in 1961 by Morris Janowitz, author of *The American Soldier* (1960) and many other works dealing with the military. Access to the seminar, however, was intentionally limited: one had to receive a personal recommendation from an IUS fellow to be considered for membership, and the IUS-sponsored anthologies were not open to unsolicited contributions.

Immediately after the *Journal of Political and Military Sociology (JPMS)* was launched, however, the IUS initiated its own quarterly journal, *Armed Forces and Society*. Some doubted whether the *JPMS* (a semiannual publication) could withstand the new competition. But *JPMS* did survive, and this book attests to the generally high quality of its articles, as 16 of the 17 chapters were originally published in that journal. The remaining chapter is an introduction intended to provide a "concise" overview of the "entire field" of military sociology, but it is so poorly written it would be rejected by any respectable journal.

The editors have classified the articles under two major headings: professional and organizational perspectives, and civil-military relations. The first group includes articles on professionalism and civil control, culture and recruitment, political orientations, and socialization; topics of the second group include military intervention in domestic politics, methodological problems in measuring intervention, and models of civil-military relations in the U.S., Brazil, Greece and East Germany. The topics are so diverse, however, that the volume has little coherence. some articles deal with a common problem, but they approach the problem in such dissimilar ways that they neither reinforce nor contradict each other. Other articles grouped in one section are less similar to each other than they are to articles grouped in another section. However, this lack of coherence is to be expected since the articles were not written for an anthology organized around a common problem or theme.

Yet, one wonders about the book's utility. For whom is it intended? According to the authors, it is meant for everyone beyond the sophomore level of college: the "more advanced undergraduate, graduate students, academics in the social sciences and humanities, and nonacademics interested in military institutions and civil-military affairs" (p. vii). How could one book satisfy such a wide audience? What is its purpose? To provide an overview of the field, the authors claim. But at what level? It cannot be used as an introductory overview, because most of the seminal articles in the field have been published elsewhere. Moreover, the introductory essay is exceptionally turgid and unnecessarily filled with jargon. Information is there, but only the most dedicated scholar will muddle through its entirety.

Neither is the book very useful as an overview of the latest research in the field. Since the editors include articles only from their own journal, they exclude important work published in other sources which are necessary to any such overview. In fact, the major limitation of the book, besides the introductory essay, is its reliance solely on six issues of the *JPMS* (two issues per year "for the past three years") for its 16 articles. That selection is too limited to provide the book any substance beyond the original journal issues.

On the other hand, for those with an eclectic interest in things military (from domestic correlates of military intervention in Africa to recruitment patterns at Sandhurst to the U.S.

military occupational structure) whose library does not subscribe to the *JPMS*, this book is a handy reference.

DAVID W. MOORE

University of New Hampshire

The Pity of it All: Polarisation of Racial and Ethnic Relations. By Leo Kuper. (Minneapolis: University of Minnesota Press, 1977. Pp. 302. $17.50.)

How do ethnic and racial tensions escalate into violence and genocide? How possible is it for racially and ethnically divided societies to avoid such overt conflict and destruction? Kuper examines these and related questions through an analysis of four societies undergoing decolonization—Algeria, Burundi, Rwanda and Zanzibar—where polarization led to massive ethnic violence and even genocide in recent years. What went wrong in each of these societies? Rejecting the notion that massive ethnic violence was inevitable in each case, Kuper asks how it came about, why it spread after several initial incidents and why it becomes so difficult to defuse such conflict after it starts.

He suggests common elements in each of his cases and offers a model which can describe, if not fully explain, the polarization process. Both structural and ideological elements interact to produce an aggregation of the population into exclusive groups with a heightened salience of sectional identities. Each side consolidates its own position reacting to measures taken by the antagonist. In such a setting, violence, once it begins, is answered with greater violence, resulting in the elimination of the middle ground. This means that intermediate groups, cross-cutting ties, reform efforts, all mechanisms for possible de-escalation of the conflict, become less important.

Kuper rejects the argument that reform efforts are doomed to inevitable failure, pointing to the many colonial societies which gained independence peacefully. "There is neither a logical nor an empirical basis for the conclusion that regimes based on violence can only be overthrown by violence. Nor is there any guarantee that violence will be effective in bringing about radical change, but only the certainty of high costs in human suffering" (p. 273). If this proposition is to be properly evaluated, what is needed is a sample of plural societies which includes cases where extreme violence of the sort Kuper describes are found, as well as societies where ethnic conflicts are resolved non-violently, or even situations where ethnic conflict is not of great importance.

Citing examples of societies in which ethnic and racial differences do not explode into extreme violence hardly informs us as to the reasons for its presence or absence. One group of hypotheses is offered in Eric Nordlinger's recent essay *Conflict Regulation in Divided Societies* (Cambridge, Mass.: Center for International Affairs, 1972) which places a primary importance on political factors. Nordlinger's hypotheses grow out of an analysis of six "open" societies in which strong ethnic divisions do not lead to violence and open conflict, but to accommodation. He suggests that at least one of the following conflict-regulating practices are found in all of his cases: stable coalition, the proportionality principle, depoliticization, the mutual veto, compromise and concession. Of greatest importance is the development of motives for avoiding conflict and conciliation on the part of leaders: the strong desire to ward off pressure from external states, to maintain or increase the level of economic well-being, to acquire or retain governmental offices and power or to avoid bloodshed among the leaders' own segment of the population. Finally, conciliatory behavior must be repeated and reinforced if it is to be continued. Were these conditions absent from the four societies which Kuper analyzes in depth? Yes, but it is harder to argue that their absence is somehow a cause of the polarization rather than evidence of it. Likewise, the set of polarizing conditions Kuper identifies seem absent from the cases Nordlinger analyzes (although in one case, Lebanon, they have developed after his essay was published).

In evaluating Kuper's study in light of Nordlinger's hypotheses, one finds an additional issue to consider: the source of an explanation for political relations among ethnic groups. Nordlinger makes a strong case for political factors involving elites and specifically rejects the importance of social explanations involving nonelites, while Kuper stresses the interaction between key political events and social structure. Choosing between them at this point is hazardous. Nordlinger's hypotheses are in a certain sense bolder and more clearly stated, while Kuper's analysis is both more comprehensive and less precise. Part of the problem is that successful explanations often require maximizing variation on the phenomenon to be explained, and both of these authors have chosen the opposite strategy of selecting cases which, despite certain diversity, are similar in a number of crucial ways. Kuper's explanation is a very effective description whose inevitability is refuted through citation of cases with different outcomes. Comparative theory building requires an equally intense analysis of cases with a wider range of outcomes to learn more precisely where in the process the different societies diverge. Such an analysis would also assist in overcoming the strong feelings of inevitability to the process which Kuper cannot fully overcome merely by the assertion that it is not so.

MARC HOWARD ROSS

Bryn Mawr College

Violence and Politics in Jamaica 1960–70: Internal Security in a Developing Country. By Terry Lacey. (Totowa, N.J.: Frank Cass, 1977. Pp. vii + 184. $22.50.)

This is an excellent narrative history of the first decade of Jamaican independence, but it is weak and uncertain as a theoretical inquiry into the causes and effects of political violence. As theory, Terry Lacey's document demonstrates again the ambiguity inherent both in violent events and in quantitative methods of their examination. Seeming to search for causes, he employs the "relative deprivation paradigm" as though already established; his conclusions, blurred and qualified as they are, come as no surprise after the bias has been revealed.

Lacey discards police reports of violent events as overly sanguine, and makes his own data collection from *Daily Gleaner,* the major newspaper of the island. He struggles to define a basic unit of measurement: "violent incident" (pp. 66–67). Setting up categories of weapons used, numbers involved, targets, etc., he develops a multi-page index of 1,800 violent incidents during the decade. Unfortunately, the method of classification, how to weigh each incident in terms of number of people involved, how to decide when an event begins and ends, is invisible, excluded from the volume. This is a significant omission; certainly a list of nearly 2,000 items might well have been included in an appendix. (By the way, there is no appendix.)

By accepting the daily newspaper as the authoritative, indeed the only, data source, Lacey becomes the prisoner of the political mood of Jamaican culture and the prevailing paradigms which motivated editors and reporters in their allocation of emphasis and priority in their newspaper columns. He thereby manages to miss what emerges in many other studies of political violence as key factors: media and group exploitation as a form of political capital and the effects of media interest in fomenting new acts.

Studies of phases of violence in both developing and industrialized countries indicate a wave-like effect. Fashions of political behavior wax and wane, much as other cultural forms do, tending to go out of style when they have lost the ability to attract attention and captivate the imagination. If the literature on issueless riots and ritualistic violence has any credibility, then this report's dependence upon the press cannot but sacrifice this dimension entirely.

On the other hand, it is clear that styles of political behavior have marked effects upon the social and political order. In this area, Lacey's work is illuminating and absorbing. His account of the post-independence cultural renaissance among the lower depths of the people is compelling and colorful. His history of the Rastafarian movement and the role of its unique musical style, reggae, are worth the price of admission. The linkage between popular music and political behavior begins to emerge, suggesting the tremendous force which cultural paradigms may have upon the political system.

Lacey brilliantly describes the importation of black militancy from the Bronx and the role of American blacks in radicalizing students and having other important effects. He presents a narrative history of post-independence linkages in Jamaica, which cannot be found elsewhere. As historian and storyteller, Lacey is resourceful and well equipped. As political scientist, he genuflects towards theory and methodology, but fails to deliver. He attempts to convince the reader that the well-springs of violence, and associated major political effects, arise from the thrust and counter-thrust of the events themselves, as a form of strategic interaction. The withdrawal of the British left a social and political vacuum which energized all kinds of claimant groups in a struggle for succession. The fight against the native colonial holdovers is soon transformed into a fight among the claimants themselves, out of which emerges a left-of-center coalition under Michael Manley, controlled by middle-class blacks in the name of the "more militant" lower classes. Its stability remains unproven, but is assisted, at least in the 1970s, by the decline of a violent political mood.

Lacey concludes on an innocuous and optimistic note: "One might hope that Jamaica would after these and current experiences find ways of avoiding open strife between her main political parties" (p. 161). He prescribes the remedy: "tackling the basic economic and social structural problems of Jamaican society" (p. 162). He calls for the elimination of "rackets and gangsters" who can "corrupt and undermine Jamaican society." One solution, he says, would be to eliminate "ganja," which would be very difficult. Another would be to legalize soft drugs, then "clean up the hard rackets, blackmail and gunmen who have hidden under the wings of the ganja industry for so long" (p. 162).

A curious blind spot. As a good political scientist, Lacey should know that these private organized groups are the real polity, out of which must come a formal and state polity—not the other way around.

H. L. NIEBURG

State University of New York, Binghamton

Politics, Language, and Thought: The Somali Experience. By David D. Laitin. (Chicago: University of Chicago Press, 1977. Pp. xii + 268. $19.00.)

How odd to encounter the shadows of Hannah Pitkin and Ludwig Wittgenstein cast upon the harsh and arid landscape of Somalia. In their shade, David Laitin finds tantalizing and unsuspected meanings, in this admirable though somewhat uneven study.

In the most original portion of the book, Laitin endeavors to apply to the Somali case the concepts of linguistic relativism which trace back to Edward Sapir and Benjamin Whorf. At a theoretical level, he weaves together in brilliant fashion the diverse perspectives of linguistic philosophy (Pitkin and Wittgenstein), sociolinguistics (Joshua Fishman and Dell Hymes), and the politics of language (Murray Edelman). He then tests a series of intriguing hypotheses through interviews and role-playing experiments with the collaboration of bilingual Somali secondary students, to see whether meaning and behavior varies by linguistic context.

The investigation, as one might imagine, was arduous both conceptually and practically. Laitin quite disarmingly describes the difficulties experienced in his field research; indeed, he was thrown out of Somalia twice. He was fortunate enough to find a quite acceptable substitute for the Somalia interviews by working in the Somali-speaking areas of northeast Kenya.

The empirical part of the inquiry yields a number of findings which do verify the broad proposition that language is much more than an interchangeable set of sound patterns, that its metaphors, categories and lexicon do influence thought and behavior in politically significant ways. This falls well short of the extreme and unsustainable position, rejected by Laitin, that language determines thought. But there is great importance in the fact that these secondary students had markedly different concepts of items as basic as the nature of the state, religion, authority and conflict resolution, depending on whether they were communicating in English or Somali.

Although Laitin is quite convincing in showing that language does make a difference, he is more inconclusive in specifying exactly what its political impact may be. He tends to identify English as linguistic vehicle with a "colonial mentality," which may well be historically true, but seems inadequate to deal with the present, much less the future. The languages, for Somalis, function in rather different realms: English for education and administration, Somali in political discourse, poetry, and intimate settings. Laitin does argue effectively that the linguistic impact depends not just on intrinsic properties of particular speech codes, but also on the contexts in which they occur.

What is really not considered is the setting of generalized multilingualism which characterizes Africa. True, Somalia appears to be one of the few exceptions to the pattern of linguistic diversity, since the overwhelming majority of citizens use Somali as their mother tongue. But even here a significant fraction of the politically active populace also know some Arabic or English (or Italian). There seems every likelihood that multilingualism will at least hold its own, and perhaps even increase despite the choice of Somali as national language. In most sub-Saharan African states, the most striking feature of language change is the spread of both regional lingua franca (such as Swahili, Lingala, Sango, or Hausa) and the dominant European language, along with retention of the numerous mother tongues. Thus the impact of linguistic relativism is quite different from those in the generally unilingual European or Western Hemisphere settings.

The other portion of the book, only loosely joined, is a political history of the critical issue of a common script for Somali, resolved only in 1972 by edict of the present military regime. Full adoption of Somali as national language was hamstrung for the first dozen years of independence by passionate discord over an agreed alphabet. The three principal contenders, Latin (the eventual winner), Arabic, and an indigenous Somali hybrid, each had their partisans, producing lines of cleavage which tended to overlap region, clan division, and religious orientation. For the first decade of independence, Somalia had a competitive democratic system, with peaceful presidential successions on two occasions in conformity to electoral verdicts. For reasons skillfully elaborated by Laitin, the parliamentary regime was unable to resolve this issue.

Although the analysis of Somali politics is solidly documented and generally persuasive, there are some arguments that merit debate. The author overstates the significance of language as a factor in elite privilege, and tends to overlook the character of the post-colonial state itself. While it was true until 1972 that command of English was a prerequisite to privileged employment in the public service, it was not just the linguistic capacity but the presumed wisdom conferred by school diplomas that was the underlying criterion. It is doubtful whether the conversion to Somali as administrative language has really altered patterns of inequality, the nature of recruitment to the public service, or the role of the state as the primary vehicle of class formation.

A related point is the exaggeration of the disabilities imposed on an African government by the retention of a European language for the written aspects of administration. While the disadvantage of any handling of public affairs in a speech code not understood by the majority of the population is evident, it is partially mitigated by the use of local languages for the oral functions of government: most African governments will receive petitioners in their own or at least regional languages, conduct local judicial proceedings in the language of the litigants, and use African languages for much of their political communication.

Finally, the arguments on the dependency-creating effects of using a borrowed language are not totally convincing. It was, after all, the English-speaking officers who adopted the Marxist-Leninist world view which at least rhetorically guides the Barre government. Indeed, the ideological vocabulary of the present regime is one of its more interesting attributes; though Laitin has given this theme interesting treatment elsewhere, one may regret that there is no mention of it in this work.

The important point, however, is that Laitin has provided us in his first book both an illuminating analysis of Somali politics, and a stimulating theoretical statement of the political role of language. If Pitkin had not already sent those who read this fascinating book to the Wittgenstein shelf of the nearest library, Laitin surely will.

CRAWFORD YOUNG
University of Wisconsin, Madison

Terrorism. By Walter Laqueur. (Boston: Little, Brown, 1977. Pp. 277. $15.00.)

In some ways this is an impressive and useful book, to which many will be indebted for some time to come. But its flaws are serious and obvious. No one, I suspect, will find it satisfying.

The subject is terrorism as an insurrectionary strategy; and the principal thesis is that terrorism is "not a new stage in guerrilla warfare, but differs from it in essential respects," and is "heir to a different tradition" (p. i). The argument is not original, though Laqueur does not mention those who have made it before him. Still, it is true and important, and Laqueur correctly emphasizes that most writers use the terms *terrorist* and *guerrilla* interchangeably.

The book exposes a vast body of literature, hitherto only vaguely known to modern students. That is its strength, and Laqueur's amazing grasp of languages, necessary for dealing with primary sources, makes it unlikely that anyone will duplicate this feat soon. We are enriched by his skill in providing details on three subjects: the writings of the "apostles of the bomb" in nineteenth- and early twentieth-century Russia, France, Italy, Germany, Poland, Ireland, the Balkans, India, etc.; terrorist movements in that period; and, finally, the reactions of academic and literary figures to these developments.

For all the valuable information that this book contains, it leaves one, however, with the impression that the author never digested the material assembled, or allowed himself time to be thoughtful about the subject. The result is a very useful catalogue (a well-written one, laced with shrewd comments) but not a penetrating sustained analysis.

There is another reason for Laqueur's problem. He does not believe a synthesis appropriate! "Generalizations [have] little validity" (p. 120), he says and hides his definition of terrorism in a footnote, "systematic use of murder, injury and destruction or the threat of such acts [for] political ends," explaining that alternative definitions (or elaborations?) "lead to endless controversies" (p. 179). Syntheses always involve risks, but can a discussion of a tradition be meaningful without defining it, particularly when one's object is to distinguish that tradition from the guerrilla one with which it has been badly confused?

An indication of Laqueur's difficulty is that he sometimes discusses writers and organizations here in exactly the same terms he has done in his companion volume, *Guerrilla* (London: Weidenfeld & Nicolson, 1977). But the issue encompasses more than the question of how one determines relevancy. It involves relationships between matters he deems relevant. Repeatedly, for example, he reminds us that the indiscriminate violence of contemporary terrorists puts them "morally and intellectually . . . light years" from nineteenth-century Russian terrorists who chiefly assassinated prominent administrative figures (p. 4). He does not try to explain the transformation, and unwittingly strengthens the position of those he castigates for thinking contemporary terrorism is sui generis. But he places the "two different species" in the same tradition; and he is right in doing so.

To unravel the paradox Laqueur will have to keep his promise to designate the essential character of terrorism. The terrorist has always aimed at altering the emotional character of a political community by shock tactics or atrocities which produce fear and especially outrage from some social elements who find themselves responding in ways *they* consider morally inappropriate. Simultaneously, enthusiasm and hope is generated in the ranks of potential supporters. While the Russian terrorists believed that discriminate assaults would be sufficient, others who were their mentors (i.e., Nechaev and Bakunin) scorned such self-imposed limits. Indeed, Laqueur's own evidence shows that most early terrorist writers (i.e., Weitling, Heinzen, and Most) championed indiscriminate violence. It would only be a matter of time before fastidious assassins would be perceived as not being true terrorists, a major theme of Camus' *The Just Assassins* which Laqueur briefly and inadequately discusses. Many nineteenth-century literary accounts, dismissed here as "caricatures" (p. 124), were also attempts to show how the potentialities of terrorist doctrine might be manifested in the personalities and aspirations of their adherents.

Laqueur's unwillingness to describe the distinctive character of the terrorist tradition seriously blemishes this book in a third way. He aptly notes that modern terrorism occurs largely in states which profess democratic principles. But the major significance of this point for Laqueur is that it demonstrates the emptiness of constantly reiterated terrorist claims that all other ways to achieve his purpose are foreclosed. Even if Laqueur is correct in seeing one aspect of this relationship, and I'm not certain that he is, there is much more to this issue. The intellectual aftermath of the French Revolution has created the breeding ground for modern terrorism. In making us believe that we can distinguish a people from its constitutional order, a door for revolutionary violence is opened. The sense that the people are morally superior disarms constitutional proponents in some respects, and simultaneously gives opponents justifications for expanding the scope of violence. Add to this the millennial beliefs which the French Revolution generated, and the genesis of modern terrorism begins to make some sense.

The final chapters discursively treat contemporary terrorism dating from the 1960s. As in the past, it is likely to be more successful when based on separatist or nationalist sentiments. Yet terrorists now display a more pronounced drift to left-wing ideologies. More than ever before, terrorism has an international character bringing together different movements, mounting attacks on foreign soil, and gathering support from diverse governments—facts which could generate serious conflict between states. Do not, however, exaggerage terrorist threats, he warns, lest constitutional liberties be undermined; "even third-rate dictatorships can put [it] down with ease" (p. 221). "Terrorism makes a tremendous noise but compared with other dangers threatening mankind [now] it seems almost irrelevant" (p. 226). Terrorism is on the wane, he concludes, but offers no evidence. A better ending would remind us that an activity which has had such a long tradition, experienced so many revivals, and is so deeply rooted in our intellectual life, will not disappear.

DAVID C. RAPOPORT

University of California, Los Angeles

Reaching and Peasant Farmer: Organization Theory and Practice in Kenya. By David K. Leonard. (Chicago: University of Chicago Press, 1977. Pp. xxi + 297. $19.00.)

David K. Leonard has achieved a major breakthrough in the field of comparative public administration generally and development administration specifically. For almost two decades, these fields have been dominated by the structural-functional model proposed by Fred Riggs (*Administration in Developing Countries* [Houghton-Mifflin, 1964]). There has been a growing disenchantment with this approach to the comparative study of administrative behavior and institutions, but only recently have viable alternatives emerged. John Armstrong (*The European Administrative Elite* [Princeton, 1973]) departed from the Riggs paradigm and demonstrated the heuristic value of a conflict approach. Armstrong also provided an imaginative and rigorous methodology for historical and cross-cultural analyses. Leonard's thesis offers a fundamental assault on the work of Riggs and his followers. Structural-functionalism is based in large part on a rejection of theories developed from Western experiences. The lesson of Leonard's scholarship is that organization theory, although developed almost exclusively from the study of bureaucracies in Western countries, is relevant and applicable to the examination of administration in the countries of Asia and Africa.

Reaching Peasant Farmers is an analysis of the extension services provided by the Kenya Ministry of Agriculture to western portions of the country. Leonard poses hypotheses from the organization theory literature and from commentaries on contemporary Kenyan politics, and then makes careful, creative and rigorous use of quantitative data to test the hypotheses. The result is a well-documented and well-reasoned explanation of the interactions within the field offices of Kenya's Ministry of Agriculture and between Kenya's civil servants and peasant farmers. Leonard is interested in the achievement of economic development and his analysis focuses on administrative processes used for that objective. In his discussion, Leonard is careful to specify which features are affected by the ethnic politics, colonial legacy, and economic opportunities unique to Kenya. In addition, Leonard can—and does—point to features of bureaucratic organizations that, based on the experience of Kenya and other jurisdictions, seem to transcend national boundaries.

The purpose of *Reaching Peasant Farmers* is not just to determine whether or not the theories of organizations apply to administration in East Africa. Leonard believes that scholars should use the lessons of their research to recommend how the subjects of study might be improved. This raises, of course, age-old questions about the proper role of the personal values of scholars. Leonard's proposals to Kenya are at times daring, but they are not uninformed or naive. For example, Leonard thoroughly and candidly presents the political and social preconditions and implications of achieving more equity in the distribution of Kenya's wealth. In so doing, he forewarns those in Kenya benefiting from the current pattern of distribution and those in the best position to thwart reform efforts. Leonard thereby demonstrates the dilemma of the scholar-advocate: in persuading the academic community of the merit of his suggestions, he may be rendering reform less likely.

This is a carefully documented and tightly argued thesis and there are few faults. One of the most noticeable is on pp. 61–63 where an analysis of friendship patterns is used to determine "the probability that a senior staff member will discriminate in favor of his fellow tribesmen." Knowing the ethnic identity of someone's friends does not provide information about how that person will or will not discriminate in the administrative setting. There are times when "organization theory" is inappropriately treated as it if were singular. The discussion of leadership on pp. 86–98, for example, would benefit from the application of contingency theory as well as exchange theory. Finally, the recommendation in chapter 6 that promotions be used as incentives for extension worker productivity, while supported by the data on worker attitudes, is seriously weakened by the absence of an analysis of turnover in the ministry and the availability of positions for promotions.

Notwithstanding a few minor lapses, this book is an unusually valuable work of scholarship. Leonard has made a contribution well worth the attention of those interested in East Africa, comparative organizations and development administration. In addition, although Leonard warns policy makers in developing countries about being hypnotized by advice from Western consultants, he is an observer from the West who is worth listening to.

DENNIS L. DRESANG

University of Wisconsin, Madison

Democracy in Plural Societies: A Comparative Exploration. By Arend Lijphart. (New Haven: Yale University Press, 1977. Pp. x + 248. $15.00.)

In *Democracy in Plural Societies* Arend Lijphart presents a unified statement and an exploratory extension of his theory of "consociational democracy," which has been among the most influential contributions to comparative politics in the last decade. Lijphart identifies a "plural society" as a society in which political divisions closely follow and especially concern salient lines of objective social differentiation. These "segmental cleavages" may involve cultural, racial, ethnic, religious, ideological, linguistic, or regional boundaries. As is widely recognized, stable democracy is difficult to sustain in such societies. Lijphart's work has emphasized the critical role which can be played by the conscious efforts and accommodative mechanisms introduced by political elites in regulating conflict in systems with potentially explosive cleavages.

In the first two chapters, Arend Lijphart describes the defining characteristics of the consociational model of democratic conflict regulation. The patterns which identify institutionalized consociational democracy include "four basic elements": (1) a "grand coalition of the political leaders of all significant segments of the plural society," (2) a rule of mutual veto to protect the minorities, (3) the use of proportionality in allocating political representation, civil service appointments, and public funds, and (4) a high degree of autonomy to each segment in its internal affairs. Various forms which these basic elements may assume—such as simple grand coalitions or shifting coalitions in which all factions come to power quite often—are discussed and exemplified, as are some of the problems which they imply or exacerbate.

In the third and fourth chapters, Lijphart provides an excellent discussion of the conditions favorable to consociational democracy: balance of power between segments, multiparty systems of varied kinds, small population size, cross-cutting cleavage structures, traditions of accommodation, federalism. Survey data from Austria, Belgium, the Netherlands, and Switzerland are used to explore the nature of the respective cleavage structures of these once or continuing consociational systems. (The reader should be aware, however, of the poor quality of the small, largely male, Swiss survey sample.) Uses of consociational practices in other types of systems are also briefly discussed. The limits of consociationalism are explored through an analysis of the tragic experience of Northern Ireland.

In the last third of the book, Lijphart attempts to break new ground through exploring experiences with consociational democracy in the Third World. The discussion is deliberately illustrative, rather than systematic. Experiences with consociational practices in the plural societies of Lebanon, Malaysia, Cyprus, Nigeria, Burundi, Indonesia, Surinam, and the Netherlands Antilles are discussed, with emphasis on the more complete use of consociationalism and greater success in regulating conflict in the first two and last two examples. In a concluding chapter Lijphart calls for more complete analysis of the costs and benefits of "consociational engineering" in the Third World.

Democracy in Plural Societies is a useful addition to the literature on democratic conflict regulation. The special role for consociational practices in dealing with situations where highly structured cleavage configurations create permanent minorities, and hence make inapplicable the model of alternating majority governments, is persuasively argued. The problems created for such arrangements by one-segment majorities and, to a lesser degree, super-fractionalization of segments, are effectively pointed out and integrated with earlier literature. The problem of homogeneity of cleavage groups within and across decentralized political subunits is particularly well treated: the creation of greater provincial homogeneity in Switzerland and Canada—but not Austria—is demonstrated with survey data; the diverging implications for consociational and cross-pressure theories are instructively analyzed. Moreover, Lijphart gives the reader a balanced feel for the interaction between the impulse and the methods of consociationalism. On one hand, elite willingness to accommodate is admittedly an essential prerequisite to consociationalism. On the other hand, once the consociational structures are in place, they provide incentives for their own extension and their support and even institutionalization by incumbent and aspirant leaders of the involved parties.

Not all of the analysis is equally satisfactory. Lijphart never really resolves the rather ambiguous position of intense class differences as a form of "segmentation." The suggestion that small countries may find it hard to turn up leaders of the requisite capacity rests on some undeveloped (and arguable) assumptions about the nature of leadership selection mechanisms and distributions of leadership potential. The role of political activists, and the constraints they can place on would-be accommodators among the leadership, gets surprisingly short shrift. Little, if any, attention is in fact paid to intrasegment political processes. Not surprisingly, the ad hoc exploratory nature of the study is most notable in the treatment of Third World societies. The failure to look at other plural societies with substantial, though not unbroken, records of democratic success is troubling: India, Ceylon, the Philippines, and Trinidad would seem to have done as well with conflict regulation as Lebanon and Malaysia. The successful application of the politics of accommodation, including development of consociational mechanism, in less "plural" cases, such as Venezuela, seems equally important.

Lijphart is certainly right, however, in calling for a careful, comparative analysis of the general relationship between regime forms and various policy outcomes in the Third World and elsewhere. We have perhaps too easily accepted the argument that because power concentration seemed necessary for the solution of Third World problems, the route of authoritarian engineering represented the most viable way.

The successes and failures of democracies, consociational and otherwise, need explicit contrast with the successes and failures of authoritarian forms, and in the light of policy choices, exogenous variables beyond regime control, and changing pressures at different development levels. Nor can we overlook the problems inherent in creating or adopting different "engineering" solutions, whether of penetrative one-party authoritarianism, stabilized military order, or consociational democratic practices. Here, once again, Arend Lijphart is directing our attention to matters which will surely engage much of the attention of students of comparative politics in the next decade.

G. BINGHAM POWELL, JR.

University of Rochester

Why Poor People Stay Poor: Urban Bias in World Development. By Michael Lipton. (Cambridge: Harvard University Press, 1977. Pp. 476. $15.00.)

> The most important class conflict in the poor countries of the world today is not between labour and capital. Nor is it between foreign and national interests. It is between the rural classes and the urban classes. The rural sector contains most of the poverty, and most of the low-cost sources of potential advance; but the urban sector contains most of the articulateness, organisation and power. So the urban classes have been able to "win" most of the rounds of the struggle with the countryside; but in so doing they have made the development process needlessly slow and unfair.

So begins this compellingly argued study of "non-disimpoverishing development," the process in which Third World countries have over the last 25 years managed roughly to double their per-capita incomes without appreciably decreasing the proportion of their populations living on the bare margin of survival. The key to the process is "urban bias": in violation of both developmental efficiency and equity, rural people—especially the poor—in these societies are being systematically deprived of income, services, facilities, and opportunities, despite Lipton's contention that it is precisely the rural sector which is most efficient in utilizing its resources, most fruitful as a focus of investment, and most essential as the basis upon which a modern industrial economy can be built.

Michael Lipton presents his case massively, demonstrating that a large gap exists between rural and urban welfare levels, incomes, and shares of investment; that the countryside is overtaxed; that urbanization drains skills and money away from the countryside; and that rural people are victimized by artifically and intentionally high prices for what they buy and low prices for what they sell. Possible counterarguments to the effect that (a) urban bias does not exist, or that (b) bias is due to some other factor, or that (c) it will disappear with time, are anticipated and rebutted. The evidence on the first point appears overwhelming. On the second, arguments that developmental biases derive from sexism, racism, regionalism, capitalism, imperialism, or the living standards and birthrates of ruralites themselves are persuasively dismissed. On the third, comparisons between contemporary Third World countries and advanced industrial societies at comparable levels of development suggest that one cannot assume that the present urban-rural gap is a transitory phenomenon, as it was in earlier-developing societies.

Most readers will take at face value, given the evidence of the rural-urban gap, the assertion that it is unfair. Lipton goes further, however; he asserts as well that the net output per unit of capital is highest in agriculture, especially among the poorer farmers, and that rapid modernization in Third World countries thus requires greater investment of resources in the small farming sector. He acknowledges that many will be unconvinced by his argument, and that beneficiaries of urban policy bias, even if convinced, will not jeopardize their own interests by rectifying the situation. On the other hand, he notes that governments "are neither monolithic nor deaf" (p. 146), and that there are those with other interests who, if alerted to the problem, might do something about it.

For this reason he offers a number of policy recommendations, directed at peasant and worker organizations, business people, politicians and planners, and foreign aid agencies. Some are realistic (publicizing the potential profitability of investment in agriculture), some may not be (increasing national agricultural research budgets tenfold), but all are sensible.

In fact, the entire book appears eminently sensible, but it leaves one with lingering doubts. The pieces of the argument add up to a convincing brief, but many of them depend upon probable (p. 272), inferential (p. 188), plausible (pp. 193, 207), or suggested (p. 284) relationships. Conclusions and positions as concrete as Lipton's call for more concrete evidence, and while one is inclined to believe him, there is a nagging lack of proof on some points.

Another doubt derives from a fuzziness in terminology, specifically regarding the difference between "urban" and "rural." The dichotomy is deceptively clear, and the author's justification is not altogether convincing (pp. 57–58). Moreover, in some instances the urban-rural and industrial-agricultural continua are mutually contradictory, and in some cases "urban" and "rural" seem to be defined in terms of education or class (p. 219), life style (p. 223), or commitment to city life (p. 150). Again, this does not necessarily put into question the basic assertion that the villagers are being discriminated against, but it is conceptually imprecise.

More unsettling is the author's analysis of urbanization. He describes this phenomenon at one point as "not very substantial or lasting" (p. 230) in light of the increase in proportion of urban population between 1920 and 1960—almost tripling in Africa and East Asia and more than doubling in Latin America and Southeast Asia—the former assertion seems unjustified. The primary macro example of the latter—the downturn of urban population in China—is due less to any universal factor than to that government's policy of forced rustication (*hsaifang*). The author is correct to note that natural increase outweighs social increase in many Third World cities, and that insofar as migration is important, it constitutes not an equilibrating influence but a drain of skills, leadership, and money from the countryside. On the other hand, I suspect that he underestimates the proportion of cityward migrants who remain permanently in the city, the value of remittances from such persons to their village relatives, and the extent of assistance by prior migrants to later arrivals in the city, while overestimating the extent to which earlier migration creates barriers to later, and the monetary sacrifices which migrants impose upon those remaining in the village. The basic argument—that urbanization accentuates rather than diminishes the urban bias in development—again remains unchallenged, but not for all the reasons adduced.

A final critical note concerns the low readability of this book. This is unfortunate given its importance both for scholars and policy makers. Perhaps the strength of urban bias makes necessary what would otherwise be analytical overkill, but the reader should be forewarned. However, it is well worth the effort. The author has revealed an important characteristic of the development process, explained it, and suggested remedies, some of which would be eminently feasible even for resource-short nations. Scholars are in his debt for the revelation and explanation, and policy makers for the suggestions as well.

JAMES W. WHITE

University of North Carolina

The Prefect in French Public Administration. By Howard Machin. (New York: St. Martin's Press, 1977. Pp. 210. $18.95.)

The study of public administration in France, once the exclusive province of jurists, has moved in recent years toward an examination of both the sociology and the politics of administrative institutions. This is a welcome change which reflects a general recognition that administrative institutions are not sui generis, but part and parcel of a complex political process which they influence and by which they are in turn influenced. Hence, to understand the role played by a particular administrative institution requires that it be examined in relationship to the larger world of which it is a part.

This is happily what Howard Machin has done in his very good book on the French prefects. Although Machin concentrates on the role of the prefects in the administrative system of the Fifth Republic, he strips away a great many myths concerning prefectoral omnipotence. Indeed, he argues that Napoleon intended no more by his creation of the prefectoral institution than that the central government should have agents who would dutifully carry out its policies in the provinces. If subsequently, particularly under the July Monarchy and the Second Empire, the prefects indulged in repression and electoral machinations, this was not sufficient for crediting them with enormous independent powers.

Machin shows that the prefects—the chief executive officers in the French *département*—who are still appointed by and responsible to the Minister of Interior, are at the center not of a rational, centralized and monolithic state apparatus but of a rather complex politico-administrative system in which there are a multitude of decision-making centers. He examines the prefect's relationship to the central authorities in Paris, to mayors, to deputies, to the field services of the ministries and to the regional institutions created by the 1964 reform. Although these relationships are not always examined systematically, Machin provides sufficient evidence that, from the variety of relations that they entertain with clients, superiors, subordinates and equals, the prefects do not emerge as the Napoleonic creatures they are still thought to be. What survives, however, is "the myth of prefectoral authority," a muth that the prefects themselves do all they can to keep alive. "As long as total reality is masked, as long as some people continue to speak of, believe in or simply attack prefectoral power, the corps can maintain its self-respect and pride. Paradoxically, by criticising prefectoral pre-eminence, the parties of the opposition are the prefects' main ally in preserving the myth" (p. 206).

Although this is Machin's conclusion, it should not be taken to imply that the prefects have become mere cogs in a machine. The great merit and interest of Machin's book resides in his description of the Fifth Republic's continual failure to take regional reform seriously, and in the role which the prefects have played in this failure. The ultimate blame falls on the politicians, for neither De Gaulle nor his successors had an abiding interest in regional reform. They did little more than create committees composed of their administrative advisors. The various committees and the conflicting pressures to which they gave rise provided the guarantee that whatever happened, the status quo would remain pretty much intact. The larger political issues were soon forgotten and the representatives of the various administrative branches got down to the job of reaching compromises that threatened no one's particular interest. The prefects were heavily involved in these negotiations and they defended their own interests rather well. Even when decisions were taken that did not accord with the interests of the prefectoral corps, the corps could retaliate *ex post facto* through its powers of application. Prefects dragged their feet when it came to setting up the Interdepartmental Conference and Regional Expansion Committees and they undermined at every turn the work of the Regional Missions, which they felt were encroaching on the jurisdictions of their own cabinets.

This is a serious, well-documented study whose interest transcends that of the prefectoral institution. It explains why an apparently strong commitment to regional reform on the part of various Fifth Republic governments has left such a sorry legacy.

EZRA N. SULEIMAN

University of California, Los Angeles

Protest and Political Consciousness. By Alan Marsh. (Beverly Hills, Calif.: Sage Publications, 1977. Pp. 272. $14.00, cloth; $6.95, paper.)

Alan Marsh is a young British social psychologist who applies his skills at survey analysis to explain differential levels of "protest potential" among the British electorate. His basic data set is a survey, conducted between October 1973 and February 1974, of 1785 Britons selected through a multistate probability sample of households. His main measure is the "protest potential scale" that merges responses to approval of various forms of protest with reports about past (or possible future) protest behavior. Marsh's book will be valuable to students of British political behavior, and deserves the attention of students of American political attitudes. Although the book has both methodological and conceptual problems, it would be well to summarize its basic findings before commenting on them.

To begin, Marsh finds what he considers to be a high level of protest potential. While 43 percent would not go beyond signing a petition, 35 percent would engage in a lawful demonstration, while 22 percent would be prepared to engage in illegal demonstrations. Age was more strongly related to protest potential than any other demographic variable, with the young scoring higher on the scale than the old. Although Marsh could not determine whether this relationship results from life-cycle effects, generational differences, or both, he was sensitive to the need to control for age throughout the remainder of his analyses.

Marsh found that Labour supporters were somewhat more likely to have high protest potential than were Conservative supporters, but partisan differences were small. Using techniques different from those employed by David Butler and Donald Stokes, Marsh found that just over half of the Britons interviewed could be placed on a Left-Right continuum, and that protest potential was greater among leftists. Marsh used responses to his Left-Right continuum questions to build a measure of level of conceptualization, and found that respondents with higher levels of conceptualization scored higher on the protest potential scale. Marsh presents some very useful data on feelings of political efficacy by using items similar to those employed by the University of Michigan Survey Research Center, and found that respondents scoring high on political effectiveness were somewhat more likely to have greater protest potential than those who felt inefficacious. Marsh then employed political trust items similar to those developed by the Michigan SRC and found that low political trust was related to high levels of protest potential.

Building upon W. G. Runciman's work, Marsh measured feelings of "relative deprivation," and found that such feelings were not widespread, and that, in any event, feelings of relative deprivation were not clearly related to scores on the protest potential scale. On the other hand, Ronald Inglehart's "materialism," "post-materialism" measure was strongly related to protest potential, with the post-materialists being much more predisposed to engage in nonlegal forms of protest. Marsh finds that "strong support exists for Inglehart's basic thesis" (p. 192), a conclusion he does not entirely square with his earlier criticism of Inglehart in the *American Political Science Review* 69:21 ff. (March, 1975).

Lastly, Marsh attempts to build an integrated model to explain differential levels of protest potential. He concludes, "we would place at the centre of a social psychological theory of protest potential what we have called the 'three-levered trigger' of political aggression; the coalescence of political efficacy, capacity for conceptualization and distrust" (p. 224). Marsh adds, "This combination of cognitive skill and affective feeling states forms the core of the basic disposition of *disrespectful* or *aggressive political competence* that we hold to be the most important psychological component of protest potential" (p. 224).

This brief summary can only spell out the broad outlines of Marsh's argument, for his analysis is based on a careful attempt to specify the conditions under which relationships obtain, and often the analysis focuses on small sub-groups. However, the main problem is not the small Ns that result when controls are introduced, but the methodological and conceptual ambiguities that make me skeptical about some of Marsh's arguments.

At a methodological level, I am somewhat confused by Marsh's scaling techniques, mainly because they vary from scale to scale. In some cases, as with the "protest potential scale," Marsh relies mainly upon Guttman scaling techniques that he himself acknowledges to be questionable. At other times, he will rely upon techniques of factor analysis to demonstrate the unidimensionality of his measures. Marsh goes to great pains to demonstrate that the standard sense of political efficacy items do not form a unidimensional scale, but decides to combine them based upon "substantive" considerations. It is to Marsh's credit that he goes to such lengths to explain his scaling techniques. But he does not explain his shifting criteria for scale construction.

At a conceptual level, I am somewhat puzzled by Marsh's frequent assaults against the thesis that Britain has a deferential political culture. Marsh incorrectly argues that Dennis Kavanagh is the only political scientist who "has seriously opposed the deference theory" (p. 31), when, in fact, it has been challenged by Butler and Stokes, as well as by Richard Rose in his more recent work. In any event, Marsh does not cogently state the deference thesis, so it is difficult to know what Marsh is attempting to falsify. Moreover, Marsh's survey contained no direct measure of deference, so his evidence on this subject is largely indirect.

More important, I am perplexed by the conceptual confusion that seems to surround Marsh's key dependent variable, "protest potential." This measure, as Marsh explains, combines attitudes toward protest and actual behavior in protest activities. Throughout his book Marsh refers to this measure in varying and possibly contradictory ways. Perhaps we should forgive Marsh his misleading title, which should read *"Protest Potential. . . ,"* but a mixture of meanings pervades the entire book. Respondents who score high on the protest potential scale are variously called "protesters" (p. 59), persons with "clearly defined predispositions to *behave* in certain ways" (p. 106), as persons whose beliefs will lead to "a clear behavioral outcome" (p. 122), as "likely to protest" (p. 200), and as showing a "preparedness to *add* protest to their range of behavioral options" (p. 223).

Despite these problems, Marsh's book provides an important challenge to conventional ways of thinking about British politics. In some respects, however, the wider meaning of Marsh's findings may best be understood in a comparative framework. Here we are fortunate, for Marsh's British data were collected as part of an eight-nation collaborative study that also included the United States, West Germany, the Netherlands, Austria, Finland, Italy, and Switzerland. If Marsh's book provides an accurate clue, we will learn a great deal about the cross-national dimensions of mass political attitudes as the results of that collaborative project emerge.

PAUL R. ABRAMSON

Michigan State University

The Distant Democracy: Social Inequality, Political Resources and Political Influence in Norway. By Willy Martinussen. (New York: John Wiley, 1977. Pp. viii + 246. $16.50.)

In this English translation of a work previously published in Norwegian under the title *Fjerndemokrateit,* Willy Martinussen, long-time associate of the Institute for Social Research and co-director of the Norwegian Electoral Research Program, sets out to account for "how systematic inequality of citizens' influence over public decisions develops and is maintained" (p. v). Martinussen's approach to this problem is organized around a theoretical model in which various contextual barriers and individual political resources, both broadly defined, play a critical role. Thus, Martinussen's analysis concentrates on the participatory impact of certain barriers and resources, especially the latter, and the possible causes and consequences of the manner in which political resources are distributed. Using data from the 1969 national election survey, this analysis is divided into three sections: Part I sets forth alternative forms of political activity undertaken by Norwegian citizens and establishes the relationships between these forms of activity on the one hand and assorted political barriers and resources on the other. Then Part II assesses the extent to which political attitudes—attitudes reflecting different facets of political alienation in particular—are interlinked and have their origins in individual circumstances or specific social contexts. Finally, Part III looks at differences in the political resources possessed by separate groups within Norwegian society and considers the significance of this situation for the political system as a whole.

The strength of this work lies in the fact that Martinussen (1) explicitly differentiates between alternative forms of both political involvement and political alienation, (2) adopts a comprehensive view of the factors conditioning individual political activity and investigates them accordingly, and (3) not only presents a wealth of information concerning citizen orientations and political activity in Norway, but also in the process raises some important questions regarding discrepancies which exist between the promise and reality of equality in contemporary democracies. With respect to political involvement, for instance, Martinussen identifies three primary channels or types of political participation—organizational activity, representational activity (electioneering and voting), and direct political action—along with two forms of "political preparedness"—i.e., information seeking (newspaper reading and following election broadcasts) and political discussion. As for political alienation, Martinussen likewise distinguishes among political detachment, apathy, and powerlessness. Although these component variables are interrelated in each case, they are nonetheless treated separately, thereby providing the basis for a much more revealing analysis than would otherwise be achieved. Martinussen finds, for example, that political apathy appears to derive more from *early* socialization experiences, a situation which can be at least partially redressed via subsequent exposure to stimuli from organizations, education, and newspaper reading, whereas political detachment and powerlessness appear to stem more directly from the *present* lack of political resources (organizational memberships and education in particular), a situation less easily remedied.

The book does suffer from several shortcomings, however. For one thing, the empirical materials are at times presented in cumbersome, even confusing manner. This problem stems from the exceptional richness and complexity of the data, but the situation is exacerbated by some awkwardness and ambiguity introduced by the translation itself. While some responsibility on this latter count must rest with the publisher for not providing better editorial assistance, a more concerted attempt on Martinussen's part to set forth his findings with greater clarity and to draw together loose strands of his analysis would have strengthened the book considerably and made it much more accessible. A second problem is Martinussen's occasional failure to recognize the limits of his data despite his otherwise obvious methodological sophistication. In discussion the extent and distribution of political activity in Norway, for instance, Martinussen concludes that individual participation is the exception rather than the rule, and that what activity does occur is conccentrated among a small minority of citizens. But as Verba and Nie's work on political activity in the U.S. has clearly shown (see *Participation in America,* chapter 2), such a conclusion is not necessarily justified and may instead represent an inferential pitfall encountered when dealing with variables pertaining to a restricted range of activities, all of which occur over a relatively short time span.

Finally, it can be noted that Martinussen does not fully achieve the goal he identifies at the outset of his book; that is, he does not manage to document empirically the critical linkages in his model—those linkages between differential rates of citizen participation and the maintenance of social inequalities. These linkages, of course, do not lend themselves to investigation by means of cross-section survey analysis. Yet without bridging the gap and then contrasting the importance of political activity to other sociopolitical processes which also have a bearing on such inequalities, definitive statements regarding the relative significance of democratic citizen participation in determining the conditions of human existence will remain elusive. This problem is particularly intriguing in light of Martinussen's own data reflecting generally high levels of confidence in public authorities and satisfaction with existing sociopolitical conditions found among the Norwegian population. These critical observations, however, do not detract from the basic merits of the book as a whole: it represents a valuable contribution to the literature on citizen orientations and political involvement in contemporary democracies.

LAWRENCE E. ROSE

University of Virginia

Tolerating Political Dissent: The Impact of High School Social Climates in the United States and West Germany. By H. Dean Nielsen. (Stockholm: Almqvist and Wiksell International, 1977. Pp. 138. Sw. kr. 49.)

Research in political socialization has characteristically invited a host of critical questions pertaining to its theory and method. Most frequently, such research is said to focus only on the product of socialization and not on the more significant process which it entails; few studies subject their particular findings to cross-national comparison to test an assumed universality of patterns; and most focus on only a single socializing agent with an expectation that is the primary source of political orientations. The resources and effort needed to address the entire battery of factors known to influence the learning process are usually sacrificed in order to concentrate on some narrower aspect. H. Dean Nielson's *Tolerating Political Dissent* represents a skillful effort to overcome many of these recurrent problems. It is both a welcome addition to the literature exploring the roots of important political attitudes and a significant contribution to the field.

The study is based on a survey of adolescents carried out by the International Association for Evaluation of Educational Achievement. The survey itself is impressive in its scope (nine participating nations), representative sampling, and meticulous instrument design. Because of the large number of original questions, Nielsen is not significantly hindered in constructing indices for his secondary analysis. The study benefits, therefore, from a solid data base from which an assessment of the impact of different school climates on the development of political tolerance can be made.

The basic objective of the book is straightforward. Nielsen seeks to locate the roots of political tolerance directly in the conducive school environment, and indirectly in the civic competence learned there. In both the United States and Germany, the expected pattern emerges, but the correlations themselves are not strong: educational climate is positively related to civics knowledge, which is in turn related to tolerance. When predictable conditioning influences are introduced as controls, student sex and religion appear to be uninfluential, but virtually all others (economic status, verbal ability, type of program, family conflict) have some impact. Consequently, Nielsen employs multiple regression to refine the model, and from this analysis parental SES and academic program disappear as independent causes of tolerance in both nations. Word knowledge (the best predictor for civics knowledge) and school climate (the second best predictor) lead to consistently higher levels of tolerance. In both countries, therefore, the cognitive path to democratic tolerance receives substantial empirical support.

Where other studies have ended with this type of conclusion, however, Nielsen's presses on to explore the dimensions of the "school climate" which may be responsible for variations in learning political information and democratic norms. He analyzes each component within the complex and concludes that "in both countries, dissent toleration appears to flourish best in schools which deemphasize 'rote and ritual,' i.e., learning names and dates and engaging in patriotic rituals" (p. 84). In West Germany, encouragement of independence is an important source of tolerance, and American students who participate in political activities are more likely to have internalized dissent norms. Moreover, the type of program students undertake is a significant factor in how competent they will become in Germany but not in the United States, suggesting that stratification effects are much stronger in the former. Finally, a path analysis of all relevant variables supports the model in which school climate produces greater competence and higher tolerance of dissent in both.

Nielsen's elaborate yet concise analysis departs in many important ways from much previous work on political learning. Perhaps the greatest strength of the work lies in its attempt to include and then disaggregate a host of critical antecedents of political orientations—from the standard SES and conditioning variables to a variety of aspects of the school environment. The latter is an especially underdeveloped and elusive aspect of the learning model, and Nielsen's careful operationalization should be instructive in how this variable might be investigated further. The examination of these factors, moreover, includes several alternative modes of analysis. Similar results from all of them lend greater credence to the basic model which, despite the large number of potentially influential factors it tests, is straightforward and simple. Some important subtle influences are tested as well, as in the comparison of individual and group perceptions of the school climate, to guard against a subjectively circular pattern of high academic performance and positive evaluation of the school environment. Finally, the interpretive sections of the book are especially well done. The relevance of the study to extant research is presented in unusually crisp and interesting propositions, and the discussion of patterns is appropriately cautious. Unlike most studies, this one includes a final section extending its findings to implications for the secondary school curriculum. Specifically, the different overall models for the two nations (a resource conversion model for the U.S. and a stratification model for West Germany) are discussed for the light they shed on differential learning patterns.

The work is not without shortcomings, although none of them are devastating. The attention to detail and inclusiveness in describing the study is overdone in chapter 3 (where, incidentally, when the data were collected is never revealed), and the lengthy discussion of measures makes one think that the analysis will never appear. More seriously, the author only occasionally notes the fact that while they are statistically significant, a good number of the correlations are very weak and the important independent variables consequently explain little of the variance in dissent tolerance. Nielsen does point this out in the conclusion, where he reports that with all controls present, school climate explained only 4 percent of the variance in tolerance, but he leaves us to ponder alone the possible causes for that fact. Without some exploration of this pattern, the importance of further refinement or new measures is left undiscussed.

Socialization research has been described as wedded either to theories of political teaching or to theories of individual learning, with virtually no attempt to integrate the intuitively related perspectives. Nielsen's book is an admirable beginning to such a synthesis with his use of multiple sources of learning (cue-giving) and variations in receptivity among students (differential perceptions of the school environment). Future research in this area can benefit from all aspects of this important effort.

MARILYN B. HOSKIN

State University of New York, Buffalo

Judicialism in Commonwealth Africa: The Role of the Courts in Government. By B. O. Nwabueze. (New York: St. Martin's Press, 1977. Pp. 324. $19.95.)

B. O. Nwabueze's learned study encompasses both more and less than the title indicates. Much of his analysis deals with the role of courts in the rest of the commonwealth—particularly in Britain—and compares their performance to that of the U.S. courts. This is sound scholarship. On Anglophonic African matters, there is rather less than one might have hoped, although what there is, is extraordinarily interesting.

Nwabueze leaves the reader in no doubt about his own position. He believes in courts as a bulwark of freedom and legitimation, while recognizing that when "the Court holds a legislative or executive act unconstitutional and void, it necessarily frustrates the will of the legislature or the executive" and thus—assuming these are elected—the courts' "checking function may be deemed undemocratic." Nevertheless, "review of small governmental acts, through the agency of an independent tribunal and in accordance with the limitations set out in the basic instrument of government, is the people's own device for the control of the power they have entrusted to their elected representatives."

The author sees the "checking function of judicial review" as exercising "a sobering influence on government by inducing in it a humane and tolerant attitude towards power." Moreover, "judges are unquestionably responsible, because they are responsive to the needs of the people, not so much to their immediate material needs, but to their need for principle in government, for those enduring values which are the ultimate end of government. . . ." This, at least, is the ideal. In examining the role of commonwealth courts in the judicial review of legislation, executive acts and bureaucratic administration, Nwabueze finds some congruence between the judicial ideal and reality in American federal courts, aided by the hardy crutch that is the Bill of Rights. In Britain, there is some cause for satisfaction at the revival of judicial activism thanks to the rediscovery of "natural justice."

In commonwealth Africa, however, with a few dramatic exceptions, the courts treat Parliament, government and even civil servants as virtually untouchable. There are various reasons why this should be so and they are engagingly, wisely and frankly discussed by Nwabueze. The fight for independence in Africa was led by politicians and quelled by the (colonial) judiciary in the name of law and order. In the post-colonial popular pantheon, judges, even "native" judges, are not heroes. Then, too, most African judges were trained as British common lawyers to whom written constitutions, social compacts, *grundnorms* all look like simple contracts, to be interpreted strictly, narrowly, without regard to anything as "unruly" as public policy and organic growth.

An even broader reason for the relatively lackluster performance of the courts in Africa may be that "Western" democracy, with its system of checks and balances, is simply incompatible with indigenous traditional concepts of power. But Nwabueze will have none of this. He wholly rejects the explanation "that democracy in a newly emerging nation ... cannot be judged by the standards of the long-established democracies." On behalf of fellow Africans he demands equal protection: not necessarily the "highest standards of democracy which may exist only in the most advanced democracies" but, rather "the standards of an average democracy."

The discussions of Nigerian, Ghanaian and Zambian cases, in particular, are fascinating. It is a commentary on the state of the judiciary in Africa—and of judicial reporting—that Nwabueze cites so few cases from the African states. While this may leave the non-African reader slightly disappointed, it is hardly the author's fault. By placing African judicialism in the commonwealth and U.S. context, even while keeping a firm grasp on the differences in socioeconomic matrix, Nwabueze has served Africa well. His book demands for Africans and all Third-World peoples the right to be first-class citizens in the community of democratic states. Just as Indians have asserted the falsity of the claim that political freedoms are of no interest to those with empty stomachs, so this book asserts that due process is of concern also to those who, perforce, sleep under bridges.

Nwabueze has served Africa in a subtle, yet important fashion. Much as the rest of the world may deplore the brutal policies of the government of South Africa, they are unlikely to assist its downfall unless convinced that the embers of freedom will not be extinguished, that African "liberation" is not merely a process of exchanging one authoritarian elite for another. Nwabueze's book (and the fact that he is currently a member of the Nigerian Constitution Commission) is strong evidence of fire beneath the ashes.

THOMAS M. FRANCK

New York University Law School

Images of the World in the Year 2000. Edited by Helmut Ornauer, Håkan Wiberg, Andrzej Siciński and Johan Galtung. (Atlantic Highlands, N.J.: Humanities Press, 1976. Pp. xvi + 729. $45.00.)

This very lengthy and expensive book details the results of an ambitious cross-national survey research project aimed at investigating images of what respondents think that the world will be like in the year 2000. The goal of the study was not to predict events that will occur through Delphi-type methodology. Rather, attitudes, expectations, hopes and anxieties concerning the future were taken to be indicators that help researchers to understand the present better. What people say and think about the future is conditioned by past experiences and present realities, and can thus serve as a diagnostic tool.

The effort grew out of public opinion studies of attitudes towards disarmament carried out in Poland in the early 1960s, and a three-nation study, "Images of a Disarmed World," completed in 1965. The "Images of the World in the Year 2000" project was born in January, 1966, and the text of the present book was completed in 1973. The cast of academics involved in the project is impressive. Eleven countries were involved in the study, although the sample from West Germany was collected much later than those from other countries and was not included in the general comparative analysis. The countries represented are diverse, ranging from Socialist Poland, Czechoslovakia and Yugoslavia, to Great Britain, Norway, Spain, Finland and the Netherlands. India and Japan were later added to the study. The national samples were not selected by any common methodology and range from an educated elite sample from one Indian state to a complex national sample of 1,001 individuals in Great Britain.

The chapters reflect the diverse interests, theories and perspectives of the researchers. Topics include the future as a neglected dimension in research, age groups and images of the future, images of women in the family, social position and future images, the future of war and peace as seen by various groups, and a wide variety of other topics. The data base is rich and each chapter is worth reading as an individual study.

The major findings from the study are gathered together, thankfully, in a chapter jointly authored by Johan Galtung, Helmut Ornauer and Håkan Wiberg. In comparing nations they find that consciousness of the future is less pronounced in "center" nations and more developed in "peripheral" nations. In other words, people in industrially developed countries don't think as much about the future as do those in less developed countries where future aspirations may make a rather unpleasant present tolerable. A similar situation exists on an optimism-pessimism dimension. People in highly industrialized countries see little reason to expect much improvement in living conditions, while those in less industrialized peripheral nations expect rapid improvement and technical-economic development. Galtung finds that respondents in the developed countries give the impression of feeling that their countries have "peaked" in terms of living standards and are now suffering from "development fatigue." When perceived prospects for world peace are questioned, it is people in the highly developed "top-dog" countries as well as centrally located individuals who are most skeptical about chances for peace. Peripheral countries and peripheral individuals are most optimistic. Ironically, skeptics about peace tend to be more cosmopolitan and educated while those hopeful for peace are frequently passive, dogmatic, alienated, and less knowledgeable about political matters.

The authors conclude their summary chapter with three general statements. The first reinforces the validity of national character and political culture concepts. The authors find the nation-state to be the most salient determinant of attitudinal differences. There is much greater variation among nations than within them. The second generalization is that people are not generally future-oriented and, when they are, they conceive of it in technical rather than in social terms. The authors attribute this to a lack of social imagination. Finally, there seems to be a contradiction between capabilities and motivations. Those respondents in more powerful positions are more capable of influencing future events but are generally skeptical about possibilities for doing so. Those without means, on the other hand, eagerly advocate and believe in change for the better.

It is difficult to arrive at a balanced assessment of the massive study that led to this book. The authors make no attempt to hide their more obvious methodological problems. Most of them are related to lack of adequate funds and national autonomy in carrying out research. Thus, the Indian sample consists of an educated elite sample of only 267 persons from ten cities in one state. The Japanese sample consists of 1,291 respondents who returned questionnaires by mail. The Japanese response rate was only 28 percent, which leads to serious questions about the characteristics that might differentiate those who responded from the 72 percent who did not. While greater care was taken in other countries, it is clear that the national samples cannot really be considered to be equivalent. This casts serious doubt on one of the primary points made by the authors: that there is much more variation between than within nations.

There are additional problems with the data. The authors admit that participants shared no common theoretical framework. In some cases the national teams that began the project were almost completely different from the teams that finished it. Time was also a factor in introducing possible error into the findings. Data were collected simultaneously in eight of the countries, but the India data were collected one year later and the Japanese data were collected two years later. Given the questionnaires' focus on optimism and pessimism as well as prospects for world peace, the flow of international events during this two-year period could well have had a major impact on responses.

On the other hand the study is important because it is the first cross-national effort of this magnitude to focus on the future. The finding that future-consciousness is poorly developed among most respondents is important, especially given the increasing need for future-orientation and policy making.

DENNIS PIRAGES

University of Maryland

Social Change and Political Participation in Turkey. By Ergun Ozbudun. (Princeton, N.J.: Princeton University Press, 1976. Pp. viii + 254. $13.50.)

The gist of this book, written in a precise and lucid style, can be condensed into one sentence: voting participation and socioeconomic development in Turkey are negatively associated. The rate of participation has fallen from 89 percent in 1950 to 64 percent in 1969, despite a notable exception: it went up from 76 percent in 1957 to 81 percent in 1961. Participation in the elections of 1973, not analyzed in this book, also went up slightly. Ergun Ozbudun—a professor of political science at Ankara University—proves with a wealth of well-analyzed statistics that indeed the least-developed north and southeastern regions of Turkey have the highest rate of voting participation. The urban areas, on the other hand, with higher literacy rates, per capita income, mass media exposure, national identification and urbanization show a lower degree of voter participation. But the lower income groups and marginal communities (shanty towns) in cities tend also to have a better voting record than other city dwellers. Ozbudun attributes the paradoxical contradiction between a relatively low level of modernization and high voting participation and social mobilization in rural areas to the persisting influence of the traditional leaders and the deferential behavior of the villagers. He believes that psychological withdrawal due to party realignment, autonomous political decision making, the preponderance of instrumental motives—and presumably the lack of wider political options—account for the low voting participation in general and in urban areas in particular.

Ozbudun's reference to psychological withdrawal as an explanation for decreasing voting participation is not very persuasive, since withdrawal occurs sporadically and is of short duration, while voting participation in Turkey has decreased steadily, indicating the existence of a long-range trend. I believe that Ozbudun's failure to explain fully the discrepancy between high socioeconomic development and low voting participation and high level of participation and traditionalism derives partly from the use of a faulty center-periphery concept of analysis, dated official statistics, and surveys carried out in 1962, rather than from his own updated empirical field investigation and interviews. Probably Ozbudun would have reached different conclusions had he viewed voting in Turkey as the consequence of a special type of social mobilization occurring within a communal system of organization, historical processes of economic and social change, as well as conflicts created by a divergence of interests and aspirations.

A new type of social mobilization alongside—or because of—alienation and anomie began late in the eighteenth century as a reaction against the development of a market economy and new class relations, the imposition of a dysfunctional legal system and alien (Western) culture upon a communal and egalitarian society sustained by its own system of political ethics. The political elites and their subgroups who promoted the new culture became a ruling group possessing all the social and cultural characteristics of the dominant economic classes in the West, but continued to legitimize their social position and authority with arguments drawn from the traditional Ottoman-Islamic culture, although the concept of government and authority in the emerging society required a new relationship to that culture and its class bases. Eventually these views came to be represented by two major political parties which embodied in an overlapping manner not only the views and interests of groups and classes but also a variety of issues stemming from the so-called dichotomy of modernism and traditionalism, which cut across cultural, social and economic affiliations, centers and peripheries, creating at times real class situations whereby economic and social status coincided with cultural and political attitudes.

Voting in the Turkish countryside should be interpreted not as a mere choice between political parties but as the expression of a collective community opinion affecting a whole range of historically determined basic social, cultural and economic conflicts. This is not a movement from periphery to the center but a multisided assimilative, transformative, and to some extent, to use Samuel Huntington's expression, "ruralizing" process to be understood in the broadest social and cultural sense of the word. Suffice it to mention that villagers define their voting as *hak aramak* (to seek justice), an expression which reflects the sociocultural history of Turkish society, and a good part of its political dynamics as well as practical demands for roads, credits, irrigation, etc.

Much of Ozbudun's interpretive deficiency derives from his uncritical acceptance of an artificial and elementary concept of center-periphery cleavages as the dynamics of Turkish politics and its use throughout his study as an analytical concept. In summary, I find Ozbudun's evaluation and interpretation of Turkish voting behavior and history rather one-sided and incomplete. Obviously these are matters of personal preference and approach which should not be exaggerated. Basically this is a good book, carefully documented and easy to read, which contains a wealth of material not only on voting, but also on Turkish politics in general. It is in fact the first concentrated and detailed in-depth topical study of an important aspect of Turkish politics. Its scholarly quality is high, and must be considered an important contribution to the growing field of Turkish studies.

KEMAL H. KARPAT

University of Wisconsin, Madison

Continuità e mutamento elettorale in Italia: Le elezioni del 20 Guigno 1976 e il sistema politico italiano. Edited by Arturo Parisi and Gianfranco Pasquino. (Bologna: Società Editrice, Il Mulino, 1977. Pp. 328. 6000 Lire, paper.)

Italian elections, and the Italian political process more generally, have been only partially integrated into the literature of comparative politics. Italy's political parties fit poorly into the available typologies, its party system defies the logic of the most systematic models crafted to capture it. In the face of these incongruities, "italinists" and comparativists have tended to develop their analyses independently.

The work under review would be useful even were it not the case that it offers data and hypotheses conducive to bridging this gap. The book contains eight essays on the Italian parliamentary elections of June 20, 1976. These were the elections in which the Italian Communist party (PCI) made major gains, moving to the threshold of formal national governmental participation. As the editors point out, however, the gains of the Communists were not the only outcome of importance for an analysis of the political system. The inability of the Socialist party (PSI) to share in the growth of the leftist electorate, and the ability of the Christian Democratic party (DC) to attract a growing share of the declining centrist and rightist vote at the expense of the small lay parties, were also of major significance. The essays address particular aspects of these developments, offering insights into the specific character, causes and possible consequences of the growing division of the electorate around two political poles. The data base for many of the essays suffers from the fact that the studies were undertaken almost immediately after the elections and thus do not draw on the kinds of detailed ecological analysis which would have allowed extensive documentation of some of the most interesting hypotheses. Nonetheless, the authors assiduously avoid the simplistic analyses which this kind of book often promotes.

The interest of the book for non-italinists derives principally from the way the authors treat the themes of continuity and change in the 1976 elections. The returns suggested to many observers that Italy had undergone an electoral "earthquake," with large sectors of the centrist and rightist electorate breaking with the allegiances which had traditionally given Italy its noteworthy electoral stability. A number of the authors suggest that this was far less the case than is assumed. Giacomo Sani persuasively argues that turnover in the electorate played at least as large a part in the shift to the left as defections of formerly center-right voters to the left parties. Barbara Bartolini analyzes the role of subcultures in postwar voting and shows clearly that they continued to play a very significant role in 1976, enabling the largest, most subculturally embedded parties to capture the votes of new voters and to hold those of old voters. And both her analysis and that of Parisi and Pasquino suggest that even from an electoral standpoint (not to speak of party organization) the PCI is not yet a catch-all party, a large portion of its new electorate coming from the working class. Together these essays, while indicating the probable continued growth of the PCI vote, highlight the continuities of Italian political life, continuities which Parisi and Pasquino point out may create the premises not only for polarization but for radicalization as well.

The articles also focus on elements of change. The decline of the PSI and of the minor "lay" parties are described in separate essays by Angelo Panebianco and Giovanna Zincone. Both authors bring electoral and organizational data to bear on arguments which suggest a gloomy future for the parties which have until recently served as buffers and bridges between the PCI and DC. Most interestingly from a comparative standpoint, the editors present a stimulating essay identifying three types of voters—"opinion," "exchange" and "belonging." They suggest, in agreement with most theories of politics in advanced industrial democracies, that there is a trend towards decline of "belonging" voters and increase of "opinion" voters, although at a considerably lower rate that would be anticipated. At the same time, however, they note the possibly growing role of "exchange" voters, especially in the face of economic crisis, and suggest that such voters, available for political solutions of the left or the right, may become the most important factor of electoral and political instability. Italy's is a political system in transition, and this book documents some of the most salient features of that process, providing ample stimulation for italinists and comparativists alike.

PETER LANGE

Harvard University

Policymaking in Contemporary Japan. Edited by T. J. Pempel. (Ithaca, N.Y., and London: Cornell University Press, 1977. Pp. 345. $17.50.)

It has become fashionable in both scholarly and publishing circles to look upon books that are the product of conferences with a mixture of tolerant cynicism and low intellectual anticipation. This view is not without justification. The quality of the chapters is certain to vary, often widely, and the thematic unity, fresh insights, and the testing of new hypotheses so bravely heralded at the outset are seldom effectively developed in practice. The present volume suffers from these common failings in some degree, but at the same time has so many and such substantial merits that it should be widely read and pondered by all serious students of comparative politics whether their predominant interests are Western or non-Western in locale and culture.

First, what does the book purport to do? T. J. Pempel and his five associates believe with respect to the study of public policy making that "it has now become virtually a truism that greater attention must be devoted to 'middle-level theory,' to the generating of theories that are empirically based but which involve more than a sngle case; studies which seek to develop or test hypotheses without becoming so abstract as to render the evolving propositions meaningless for those concerned with the explanation or prediction of day-to-day activity" (p. 14). They set forth to do this through five detailed and very well-wrought studies of policy-making episodes or major issues in recent and current Japanese politics: the resumption in 1972 of diplomatic relations with the People's Republic of China (Haruhiro Fukui) the long struggle during the 1950s and 60s by the 850,000-odd Japanese families forcibly repatriated to the homeland after World War II to gain public recompense for their suffering and losses (John Creighton Campbell), the annual donnybrook that attends the decision regarding the level at which the central government's budget will support the producers' price for rice (Michael W. Donnelly), the intensely controversial issue of pollution and policy making (Margaret A. McKean), and public policy making in higher education (T. J. Pempel). These more substantive case studies are embellished by Fukui's excellent review of the literature relating to public policy and decision making in Japan, and an intricate and interesting methodological statement by Bradley M. Richardson that suggests three prime "organizing perspectives" or "rationales" for policy-making studies in Japan (and, by extension, elsewhere): interest, political advantage, and institutional position. Pempel provides the book's other major methodological schema in his chapter on higher education where he identifies and explicates three dominant patterns of Japanese policy making: "camp conflict, pressure group pluralism, and incrementalism." Pempel also supplies brief introductory and concluding chapters. There is an excellent annotated bibliography and an index.

Why is the book notable? Primarily, I should say, because it goes far toward raising the intellectual level of the literature on public policy making and decision making in Japan to a plane of insight and sophistication far more comparable to that of the field as a whole. It demolishes the entire array of simplicities that have for too long passed as revealed wisdom concerning the basic issues of "Who decides?" "Who possesses power or influence?" and "How are these exerted?" The new verities are not the discoveries of this particular set of authors; others have seen and written of them too. But never before has the case been made in such convincing detail and in so wide a variety of relevant and important contexts.

The classic triumvirate of power-wielders in contemporary Japan has normally been depicted as the higher bureaucracy, the top leadership of the dominant conservative party, and "Big Business." For some time now, more perceptive scholars both abroad and in Japan have been pointing out the diffuseness, the internal and external antagonisms and competitiveness, and the limitations of influence that characterize these three categories individually and collectively. But somehow the real complexity and variety of the policy-making process in Japan did not emerge. The situation was somewhat similar to that in the "community power" literature in this country before Dahl's New Haven study. This circumstance has now been rectified. Pempel and his colleagues make clear the existence and the importance of a variety of "relevant publics" relative to the issues under consideration; how they organize for the fray; the modes of access and pressure they employ in their quests for satisfaction; and the relative helplessness of the higher bureaucracy, and ruling party's leadership, and big business in some contexts, in contrast to their enormous power in others. Even actors as legendarily impotent as local governments, the courts, or the opposition parties are shown in some circumstances to possess substantial power and a real capacity to influence major governmental decisions.

What emerges, therefore, is a new and much more convincing degree of pluralism in the governance of Japan. While it is not necessarily democratic in content, it is probably democratic in tendency. What does not emerge, however, is any new or similarly convincing formulation of that "middle-level theory" capable of conferring unity on multiplicity or of predicting day-to-day activity in the policy-making realm that the authors set forth to identify. The individual case studies are not really informed by any shared mode of analysis—even those proposed by their coauthors Pempel and Richardson—nor do they lead to any positive new theories or generalizations. But does not this reflect the somewhat disappointing state of the art in general?

It is of much greater moment to note that this group of younger scholars—their average age is well under 40—has written a book that most impressively embodies a development that some of us have long awaited: the integration of sound area training and skills with a level of methodological and theoretical sophistication of which the discipline can be proud.

ROBERT E. WARD

Stanford University

Italy at the Polls: The Parliamentary Elections of 1976. Edited by Howard R. Penniman. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1977. Pp. xvi + 386. $5.75, paper.)

Studies of elections usually present a static and narrow picture, a mere segment of more complex political phenomena. This collection of essays by some well-known specialists in Italian politics (LaPalombara, Putnam, Barnes, Di Palma) and newer names (Wertman, Sani, Leonardi, Porter, Hellman) offers a lively and rich analysis not only of the 1976 parliamentary elections, but also of the intricate developments which have characterized the Italian political landscape in the postwar period. This book also provides a revealing synthesis of the major political developments of the past 20 years which have accompanied Italy's transformation from an agrarian society to an advanced industrial society.

The great value of *Italy at the Polls* is that it provides a solid context for an understanding of the present paradox of Italian politics. In a country not unlike Great Britain in its basic socioeconomic structure and income distribution, one finds the Communist party obtaining 34 percent of the popular vote by 1976—when in fact on the basis of economic indicators one would have expected a steady erosion of Communist strength in recent years. While the book provides no definitive explanation for this paradox, it does give a real sense of the uniqueness as well as the universality of the Italian situation in accounting for the linkages between political ideology, socioeconomic change, and electoral behavior.

Given its broad perspective, this book will interest all students of comparative government, not merely specialists in Italian politics. For instance, one of the essays analyzes the proportional representation system and related electoral dynamics in Italy, thus providing a vivid illustration of the intricacies of PR. Of equal interest is the phenomenon of small parties caught up in a multiparty system with two hegemonial parties. Students of leftist politics will find fascinating material on the behavior of a communist party in preparing for elections and in relating ideology to tactics.

As might be expected, the Communist party stands out as the main focus of the various essays in this book, reflecting the party's emergence by 1976 in the context of a radical shift in attitudes which has been occurring for several years, confirming attitudinal patterns which have been evident at the elite level for at least a decade. For instance, in a study of elite political culture based on data collected in the late 1960s (*The Beliefs of Politicians: Ideology, Conflict, and Democracy in Britain and Italy*, Yale University Press, 1973), Robert D. Putnam concluded that even the most ideological politicians in Italy were not hostile to the practices of pluralism. Putnam's findings suggested that, in effect, there was an increasing tendency on all sides of the Italian political spectrum to accept opponents as legitimate. The 1976 elections and their aftermath fully confirm this pattern: thus the moderation of the Communist party and its commitment to the democratic process was matched by the willingness of the Christian Democrats to respond to Communist overtures for a pragmatic political compromise aimed at specific policy goals despite underlying ideological differences.

From this study also emerges a sharp picture of a significant consensus on foreign policy. All of the major parties, including the Communists, seem to be solidly committed to Italy's alignments within the European Community and the Atlantic Alliance. Thus, for the first time in the postwar period, in the 1976 elections foreign policy was no longer a salient political issue—having become a shared policy goal for all parties (p. 288). Another significant trend highlighted here is the transformation of the Italian political culture. Active and discriminating political participation has taken the place of the political apathy painted by Almond and Verba in *The Civic Culture* (Princeton University Press, 1963). There is also confirmation of a trend towards a de facto two-party system similar to Britain's. Although already foreseen by Italian commentators in the mid-1960s (cf. Giorgio Galli, *Il Bipartitismo Imperfetto,* Bologna: Il Mulino, 1966), the emergence of a two-party system will come as a surprise—as most texts in comparative government still refer to Italy as an extreme example of fractionalized multi-party politics.

There are some major problems in this book; little attempt is made to relate the conclusions of various essays. All too often, the same basic story of the 1976 elections is repeated by various authors—despite the fact that a good summary is provided in the preface and in the concluding chapter. Also, at times the reader is faced with contradictory conclusions by two different authors without any attempt at explanation. For instance, while one author demonstrates that most candidates conduct "highly personalized campaigns" (p. 74), another author concludes that "personal traits" are insignificant in Italian elections (p. 98). The book is rich with statistical data and charts which provide an excellent empirical base for understanding some of the subleties of Italian politics. At times, however, the charts are not matched by adequate analyses of their significance. For example, one of the essays presents data on attitudes towards religion indicating that "the pool from which in the past came a considerable proportion of Christian Democratic votes is getting smaller" (p. 118). No explanation is given, however, of the fact that the Christian Democratic party has essentially retained the same level of electoral support (approximately 38–40 percent) since 1953.

The book does not include comprehensive charts showing electoral results for each party since 1946, and without the overall picture some of the specialized charts lose their impact. On a minor point: in several places throughout the book one finds misspellings and incorrect translations of Italian words. Somewhat inexplicably, the book also contains a serious omission: there is no real discussion of the trade unions despite their preponderant role within the economy and the political system, and their complex and delicate relationship with major political parties.

Unlike many studies on political behavior, this book provides useful and pertinent discussions on economic issues and on the international context which affect Italy and the parties. Most significant aspects of political life, directly or indirectly related to the 1976 elections (from analyses of each party to electoral strategy and the role of the media), are analyzed in a sophisticated manner and yet with a degree of detail and explanation that bring the subject within the grasp of the nonspecialist. And this is a most significant contribution. *Italy at the Polls* goes beyond all other electoral studies on Italy, both in English and Italian, providing not only an updating but also the most comprehensive analysis of the political parties and their role within a changing political system.

PRIMO VANNICELLI

University of Massachusetts, Boston

The Comparative Study of Political Elites. By Robert D. Putnam. (Englewood Cliffs, N.J.: Prentice Hall, 1976. Pp. 242. $8.25, paper.)

This book offers a successful synthesis of research and theory about political elites. It makes what has long been called a subfield really begin to look like one. Written for both undergraduates and scholars, the monograph is already widely used by both: students are attracted to the straightforward prose and vivid figures, scholars to the systematic and comprehensive integration of literature. Anyone studying political elites will readily recognize this work as the best overview in the shop.

The only readers likely to be disappointed are those who demand general theories covering all elites and all political systems. Instead, Putnam's study deals mainly with national politicians. It sometimes refers to civil servants and the military, but others (such as business or trade union leaders) are excluded. Likewise, the data and major examples come primarily from European and American sources, although there are many shorter references to Third World political systems. Actually, the focus on politicians is, I believe, a considerable strength which provides theoretical coherence and empirical depth. And the focus on Europe and America would appear inevitable since that is where most available data are found.

Although the book is intended to be synthetic rather than original, its synthesis is woven with a creativity and clarity of design that produces results more useful for cross-national analysis than most sources being synthesized. This is evident in the introduction's fertile typology of political stratification and carries through to the end where an extensive discussion of elite transformation surveys the impact of industrialization, revolution and technology upon the character of political leadership.

In the same vein, an analysis of class, education and power uses simple causal diagrams with a lucidity which encourages students to think about subtle relationships among variables. This is presented in a chapter on elites and social structure which also foreshadows two other features characteristic of the book: Putnam likes to ask "Why?"—Why do upper-status groups win the lion's share of seats among the political elite? And he doesn't flinch at "So What?"—What are the consequences of social bias in leadership selection?

The chapter on motives, beliefs and their sources is very nice indeed, especially its treatment of elements in elite belief systems. This draws heavily on Putnam's own British and Italian research and reflects his assumption that cognitive factors like attitudes and values play a central role in elite behavior. It seems no accident that he devotes three times more space to this topic than to motivations; though it should be noted that the motivation section includes a useful review of evidence for and against Lasswell's compensatory hypothesis. A summary of studies about origins of elite belief systems rounds out the chapter by again bringing clarity to an often muddled topic.

Every book, however, has its weaknesses. The soft spots are found in elite recruitment and elite-mass linkages. Thus, the author's recruitment chapter mirrors shortcomings of the field: superabundant data, but few generalizations. This descriptive bent tends, in cross-national perspective, to produce somewhat tedious strings of examples and exceptions in proportion to the number of political systems covered. His analysis of elite-mass linkages suffers from the opposite difficulty: a potpourri of various general points and claims which cannot be developed satisfactorily because there are so few empirical studies of the subject. Nevertheless, Putnam manages to introduce a bit of sparkle by raising the "so what" question for recruitment and by examining assumptions about the role of electoral competition in elite-mass linkages.

Perhaps the best chapter is the analysis of elite consensus and unity which assesses empirical questions underlying polemics between elitists and pluralists. Here we see plainly another valuable characteristic of the author's approach: his determination to avoid foreclosing empirical questions, such as elite cohesion, by definition or dogmatic assertion. Putnam begins with the sensible supposition that elite integration is a variable and sets out to identify and examine its dimensions. His analysis of social homogeneity and recruitment as foundations for unity may be too cautious, possibly because such data have often been misused by left-wing commentators. But the discussion of value consensus and institutional linkage dimensions makes some very important distinctions which are often overlooked. And the coda is equally welcome: a succinct and intelligent consideration of the consequences of elite integration for stable democracy on the one hand and for tendencies toward oligarchy on the other.

The author of this overview of research about political elites is well suited to his task. Putnam has an eye for finding important questions in a morass of data and a talent for reassembling and reinterpreting the data to address these questions. He also has prodigious energy (the excellent bibliography contains more than 600 items) which occasionally produces unnecessary billows of examples. But Putnam always compensates his readers handsomely by drawing their attention to clever and significant generalizations. It is a good book.

DONALD D. SEARING

University of North Carolina, Chapel Hill

Political Behavior of Adolescents in China: The Cultural Revolution in Kwangchow. By David M. Raddock. (Tucson: University of Arizona Press, 1977. Pp. vii + 242. $8.95, cloth; $4.50, paper.)

The actions of student Red Guards during the 1966–68 Cultural Revolution in China cry out for psychological explanation. The decision whether to participate or to stay at home might be explained by a person's estimates of the costs and benefits of these alternatives, but the venom of Red Guard attacks against teachers and cadres and the violence of battles between Red Guard factions cannot be explained by rational choice alone.

Yet in view of the theoretical and empirical difficulties of "personality and politics" research and the practical data problems of contemporary China scholarship, it is only the bravest and most thick-skinned scholar who would venture a study of the impact of psychological predispositions on Red Guard behavior. We should be grateful not just for

David Raddock's courage, but also for his analytical judgment. His book represents a significant advance in the study of Chinese personality and politics and a contribution to the broader field of personality and politics as well.

On the basis of lengthy quasi-psychoanalytic interviews with 37 adolescent refugees from Kwangtung province, Raddock finds that an individual's choice about how to participate in Red Guard activities was influenced by family socialization, particularly, in the case of males, the father-son relationship. The retrospective accounts of his respondents also lead Raddock to conclude that environmental changes have transformed family socialization; the traditional hierarchical authority relationships between father and son are gradually being replaced by more horizontal relationships. The book compares the participation patterns of youth raised in these two types of Chinese families.

While young people from both types of families sought to get involved in the Red Guard movement, those from families characterized by more horizontal authority relations were more likely to sustain their participation because they did not confront the guilt problems of those from families characterized by vertical father-son relations. In "horizontal families" the status gap between father and son was narrowed and there was rivalry between them. Although not all sons from such families chose to participate in the Cultural Revolution, those with fathers who accepted political activity as a legitimate form of achievement and who could identify vicariously with their sons' political achievements, did become Red Guards. The sons' deep motivations for activism were explained by their desire to surpass their fathers.

Fewer sons from "vertical families" tried to join in the movement, and their attempts at participation were less successful. If such a son attempted to participate, his behavior was explained as an act of defiance against his father; as such it was "susceptible to being aborted by the guilt engendered in defying father and by a greater need on the part of the young individual to have his behavior reinforced and supported by the outer social environment." If such support was not forthcoming, the failed rebel from the "vertical family" was forced to revert to his father's identity and "drop out" of the Cultural Revolution.

This study brings us, as the Chinese might say, "Three Steps Forward" in our analysis of the links between personality and political outcomes in the Chinese setting. First, Raddock's dependent variable is the choice to participate in a specific, well-defined movement when a political channel of expression was available, rather than some amorphous social trend or behavior across an undefined span of time. Second, the author focuses on *change* in patterns of family socialization instead of assuming the persistence of traditional patterns. And third, he is sensitive to the importance of the wider social-political environment and its interactive effects with family socialization and personality. Raddock in particular concentrates on school peer relations (and, implicitly, on educational and occupational recruitment policies) as they mediate between family socialization and Cultural Revolution behavior. He argues persuasively that many young persons with nonproletarian family labels who try to break from "politically backward" fathers are forced back on an identity with the father because of a school environment in which they are suspect, by virtue of being without a pure revolutionary pedigree. Even if they "draw a clear class line" to distance themselves from their fathers, they are unlikely to win acceptance from their peers, not to mention admission to the Youth League or to college.

Despite these insights, the book does have a few problems. It never comes to grip with the fact that most Red Guard activity (and almost all of its serious violence) was directed not at adult authority figures such as teachers or cadres, but at rival Red Guard factions. Raddock finds it difficult to specify the links between father-son relations and Red Guard factional battles.

The book also fails to explain the influence of other variables such as sex, parental political values, and class background on family socialization and political behavior. The author limits his analysis (albeit apologetically) to the father-son relationship even though some of the most vehement Red Guards were young women, and one of Raddock's cases demonstrates the strong impact of the mother on the son's behavior. And we are never presented with a clear description of vertical and horizontal father-son relations so that we might consider the impact of these other variables. This analytical fuzziness makes the argument about the strong effect of authority relations less credible, especially because several of the case studies suggest that class label and parents' political views explain as much or more of a Red Guard's behavior as his relations with his father.

The book is clearly written and filled with fascinating anecdotes of Chinese family, school, and political life. One takes away from the book such poignant images as that of the boy who spent his summer vacation in a university library poring over stories about Mao Tse-tung and other communist leaders in order to learn the secret of their success.

SUSAN L. SHIRK

University of California, San Diego

Quest for Peace: United States-Israel Relations and the Arab-Isareli Conflict. By Bernard Reich. (New Brunswick, N.J.: Transaction Books, 1977. Pp. 495. $24.95.)

Bernard Reich can lay claim to being America's foremost chronicler of events of the Arab-Israeli conflict. He has all the necessary qualifications: encyclopedic knowledge of the setting, a fierce determination to be fair-minded and to avoid imposing his own interpretations on events, a dedication to examining all the pertinent literature and to reporting his material with meticulous care. Only once did I feel the compulsion to check one of his 1232 notes (which cover 201 pages of the text) and that was to check for an unintended omission. The result is a book rich in detail and hence an invaluable reference work for students of the area. Much of the detail is too tedious for entertaining reading, but there undoubtedly are students who will want to know the dates of Henry Kissinger's various visits to Amman. These and countless other such details are easily available in the book.

Reich has provided another vital service for students of the area. While carefully circumscribing his own analytic boundaries, his footnotes alert the reader to a spectrum of interpretations of the event being described. Varying interpretations by major analysts are summarized in the text, always fairly. There were some surprising omissions, however. Kennett Love's *Suez: The Twice-fought War* and the *London Sunday Times* Insight Team's *The Yom Kippur War* offer some of the most challenging theses in the literature concerning these events, and their neglect here is puzzling. The Love book is not included in the bibliography.

But how does one provide a detached and fair description of events which the participants see so differently? Reich attempts to deal with this problem by giving the contrasting views of the participants. Given his limited willingness to interpret, this leads him quite naturally to take statements by diplomats, politicians and even official communiques at face value. The Israeli view of an event, for example, is typically represented by a statement made at the time by Yitzhak Rabin or Abba Eban. Without question, such verbal data is important evidence for inferring Israel's "view." But, standing alone, such statements can only distort. They are always purposive and the analyst owes the reader an estimate of what the purpose is, what has been stressed, what has been omitted and why. Reich is unwilling to engage in such an unavoidably judgmental endeavor. The result is a great deal of undigested primary data which can be useful for the specialist but will mislead the lay reader.

The problem is compounded by the asymmetry in the use of the device of official statements to give the views of the various actors involved. Reich is very careful to allow quotes of Israelis form the basis for the Israeli view. He is less so for the United States and much less so for the Arabs. The real hiatus in approach, however, is not between the treatment of Israelis and Arabs but rather between the treatment of the Soviets and that of other actors. More frequently than not, Soviet views and intentions are asserted, not illustrated by Soviet statements. The Soviet Union thus pictured is of mid-cold war vintage. By having Czechoslovakia sell arms to Egypt "the Soviet Union had entered the Middle East and has reached the southern tier without regard to the Western-supported northern tier defense system" (p. 30). Kosygin went to Glassboro in 1967 "to avenge the Arab defeat and to salvage the Russian position and investment in the area" (p. 84). The Soviet effort to expedite a move toward Middle East peace in 1968 was in part "designed to divert attention from its invasion of Czechoslovakia, to show support for the Arabs, and to produce tension between the United States and Israel by emphasizing their different approaches to peace and their divergence on specifics" (p. 91). The Soviets were prevented by Kissinger from playing the role of "spoiler" in 1974 and 1975 (p. 404) but in 1976 in the UN they were executing a "diplomacy of polarization" by which they "sought to link the United States with Israel in the view of Arabs and the international community" (p. 419). Presumably Soviet purpose is self-evident and need not be developed with the same care and caution as is the Israeli.

Reich describes briefly all the major diplomatic events in the area since the June War of 1967. With only one exception, however, he avoids the arena of conflicting interpretations. The exception is the Kissinger step by step diplomacy culminating in Sinai II. Sinai II was "a significant accomplishment and constituted a major milestone in the U.S.-Israeli relationship, U.S. Middle East policy, and the Arab-Israeli conflict" (p. 349). Like his judgment of Soviet intentions, this judgment is asserted. The reader does learn that the judgment is sharply challenged in the literature (p. 405) but the basis of the disagreement is left unexplored.

In the final chapter Reich focuses directly on the American-Israeli relationship. Is it a special relationship? Is the defense of Israel in the American national interest? How effective is the Jewish lobby? Given the close relationship, why has there not been a treaty guarantee of Israel's security? These are all important and fascinating questions. But the treatment here is descriptive and essentially nonanalytic. Whether it is in our national interest to defend Israel, for example, is a question that necessitates a definition of national interest and an identification of a set of indicators. Reich treats the subject mainly by describing the rhythm of American military grants and sales to Israel and by quoting people who say it is or is not in our national interest to invest heavily in a strong Israel.

This is not the book to look to for a fresh and insightful analysis of Middle Eastern diplomacy. It is instead a most useful bibliographic and reference work, truly a chronicle of events.

RICHARD COTTAM

University of Pittsburgh

Guinea: The Mobilization of a People. By Claude Rivière. Translated by Virginia Thompson and Richard Adloff. (Ithaca: Cornell University Press, 1977. Pp. 262. $16.50.)

Claude Rivière's book, *Guinea: The Mobilization of a People,* is an insightful description of the Republic of Guinea from the perspective of a well-informed French social scientist. Virginia Thompson and Richard Adloff have prepared a very readable translation of the original French. The author examines the peoples, history, politics, international relations, economy and extent of national integration in the country. Four of the seven chapters are actually revisions of articles published by the author between 1971 and 1975 in *Revue Française d'Etudes Politiques Africaines.* It is a worthy addition to the series edited by Gwendolen Carter entitled "Africa in the Modern World." Nevertheless, its substance will evoke some controversy and its methodology some criticism.

The controversy will derive from Rivière's pessimistic assessment of Guinea's progress toward the good society. For example, the author concludes that "overpoliticization, which was simply burdensome in its early stages, has now alienated the population" (p. 100); that with state control of the economy came "the partial paralysis of enterprises, the breakdown of the cooperative system, the stagnation of plantations and rice culture, and the embezzlement of public funds to the detriment of the accumulation of national capital and productive activities" (p. 173); and, that in reality those who share power with Touré "find that the ideology they proclaim serves—either unwittingly or deliberately—as a cloak for their growing transformation into a bourgeoisie" (p. 243). The controversy over such points is likely to be raised both at the level of values, i.e., over whether socialism is a worthy objective, and at the level of substance, i.e., over whether the evidence is reliable and valid. Although it is not possible to evaluate the former empirically, the latter may be assessed partly on the basis of the methods used to collect and present data.

It is with the methodology that some criticism is appropriate. Three aspects warrant particular attention. First, there is no consistently employed theoretical framework. The author begins the book with the subtitle "The Mobilization of a People" and ends it by equating Guinea with Apter's "mobilization system" (p. 244). Yet, the concept "mobilization" is rarely mentioned in the text and the notion of a "mobilization system" is not introduced until the next to last paragraph of the book. Rivière also employs a class framework, but, once again, it is never fully developed nor used consistently. The dominant class he identifies in at least four different ways, including "bureaucratic bourgeoisie" (p. 129), "administrative and political bourgeoisie" (p. 130), "state bourgeoisie" (p. 239) and, simply, "bourgeoisie" (p. 243). The use of bits and pieces of theoretical frameworks may reduce the consistency of bias, but it does not facilitate an assessment of the meaning and interrelationship of evidence.

Second, explanations are propounded by Rivière often based on peculiar psychological conditions that are more insightful suggestions than substantiated facts. For example, Rivière accounts for the acceptance of the PDG's ideology in terms of its purported psychological effects, "its sublimation of the Guinean's frustrated desire for dignity, its projection of his basic ego, and the escape it offers him into a new and apparently rational kind of mythology" (p. 93). And, Touré's political behavior toward neighboring countries is explained in terms of his "constant preoccupation with a possible attack by the imperialists and his obsessive fear of a domestic counter revolution. . ." (p. 88). There may be some validity to the psychological conditions claimed, but greater substantiation of their existence and function would strengthen the explanations.

Third, the more rigorous tools for data collection in the social sciences are not employed. There is little indication that data were collected systematically. Once again, this does not invalidate conclusions, but it makes some more open to question.

Despite these three problems, Rivière's book contains useful insights and fascinating information. It is very readable and relatively objective. Guinea's experiences are surprisingly similar to those of other African countries seeking to build some form of socialist society. The parallels implicit in Rivière's work may stimulate comparative study and the building of more general knowledge on the process of socialist construction.

DEAN E. MCHENRY, JR.

University of Illinois

The Politics of Bhutan. By Leo E. Rose. (Ithaca, N.Y.: Cornell University Press, 1977. Pp. 237. $19.50.)

No country in Asia has received less attention than the small and largely inaccessible Himalayan kingdom of Bhutan. In fact, this is a country which is virtually "data-free" in regard to both secondary and primary sources. There are no central archives, no newspapers, only "meager" official files (available on a "very selective basis" to Leo Rose), and only one periodical, the official *Kuensel,* which has existed only since 1967. As a result, Rose's study "depended almost totally upon personal interviews not only for interpretation of developments, but even for the most basic facts." Moreover, since Bhutan has virtually no nonofficial educated elite, the interviews were held largely with government officials.Under such circumstances it is perhaps understandable why the author is modest in his claims: "The analyses and hypotheses presented here . . . are highly impressionistic. . ." (pp. 10–11).

The book is organized as follows. The first chapter presents an historical background, along with a smattering of information concerning Bhutan's contemporary society and economy. The second chapter is a somewhat expanded version of Rose's earlier article on Bhutan's foreign relations (*Pacific Affairs,* Summer 1974). The most informative part of the book is Chapter 2, which focuses on the policy process and on changes in several areas of public policy since 1950. Chapters 4 and 5, which deal with the constitutional system and with administration, are fairly broad in scope and contribute to the process and policy discussions, as does the final chapter on the process of change.

Within this framework Rose provides "an introduction to Bhutanese politics" (p. 11); and indeed it should be viewed as no more than that. Nevertheless, in a descriptive sense the study is important. In particular, several themes emerge. First, if there is such a thing as a "traditional" society, Bhutan surely must be it. The narrow elite rules "virtually unencumbered by the need to cater to popular prejudices, preferences, and interests" (p. 213). The economy is almost totally agricultural, and there is little "propensity toward mobility" (p. 53). Until very recently, even the "literate political elite" was unfamiliar with "the concepts of modernization and development" (p. 212).

Although Bhutan continues to be largely a traditional society, a second theme revolves around the fact that Bhutan since the mid-1950s has embarked upon an "ambitious program of guided political, economic, and administrative change" (p. 211). Rose's analysis suggests that change has occurred, but its extent and depth remain unclear in spite of substantial emphasis on the topic. He may also overstate the potential for long-term, orderly change in Bhutan. Nevertheless, he clearly feels that the continued success of this reform program as well as the stability (or perhaps even the survival) of the "highly personalized" and "unstructured factional" political system depend heavily on the monarch (p. 123). Such dependence on the occupant of one hereditary position suggests the existence of a rather fragile political system; and one may view this as a third, although to a substantial degree implicit, theme of Rose's study.

A final theme is the dominance of India in the evolution of Bhutan, particularly after relations between India and China began to deteriorate in the mid-1950s. Virtually all "modernization" programs have depended heavily on India for both financial and staff resources. Bhutan's foreign policy is based on an "across-the-board alignment with India" (p. 104). And Rose refers to the "omnipresence of the Indian Army" in the form of large training units, more-than-adequate support forces, road engineering and construction units, and perhaps other units as well (p. 203). In fact, he on occasion refers to Bhutan's "dependency relationship" with India" (p. 188). Nevertheless, he seems to have difficulty squarely facing, presenting, and accepting the existence of this Indian dominance. In this regard, it should be noted explicitly that a study which depends so heavily on interviews offers substantial risk of observer bias; and Rose in part may be reflecting an overidentification with the views, goals, and sensitivities of the Bhutanese on this and other issues mentioned above. But such reservations aside, this book is a welcome addition to the South Asian literature.

JOSEPH W. DODD

University of Tennessee, Knoxville

The Dynamics of Public Policy: A Comparative Analysis. Edited by Richard Rose. (Beverly Hills, Calif.: Sage Publications, 1976. Pp. 268. $15.00, cloth; $6.95, paper.)

Public policy analysis in general and comparative public policy in particular, has come of age in political science and this volume by Richard Rose is exciting evidence in support of such a view. Indeed, one could hypothesize that the next decade of research in political science will be the period of policy studies.

The early and middle stages of the behavioral revolution in political science concentrated on what systems analysts refer to as the "input side"; that is, the analysis of political attitudes, socialization, voting behavior, etc. Ironically much of this research was not behavioral per se but sought to explain one set of political attitudes via another set of attitudes. With the exception of the voting studies, political behavior as such did not constitute as important a dimension in political science as one might have suspected. Moreover, much of this research was at micro-level (i.e., the level of individuals).

Recently, however, a significant proportion of behavioral research has shifted to the public policy or "output side" of the political system. Comparative policy analysis of the American states was pioneered by Ira Sharkansky, Richard Hofferbert and Thomas Dye, to name only a few of the major contributors. Their groundbreaking empirical work was paralleled by significant conceptual and theoretical work on policy models and typologies by Lewis Froman, Theodore Lowi and Robert Salisbury.

In the last few years a number of cross-national policy studies have been completed and among the best of these are the important work by Heidenheimer, Heclo and Adams, *The Politics of Social Choice in Europe and America,* Heclo's *Modern Social Politics in Britain and Sweden,* and Brunner and Brewer's edited conceptual work on process and policy *Political Development and Change.* Richard Rose's edited work, *The Dynamics of Public Policy,* is a recent addition to this growing body of literature on comparative public policy.

The conceptual framework of the volume is set out by Richard Rose in the introductory chapter. He seeks to define the basic concepts required to understand the dynamics of public policy by identifying four alternative models of change; namely, static, cyclical, linear and discontinuous. He also proposes a model of the political process which contains a series of 11 discrete steps.

The chapters which follow contain several case studies, with an attempt to have them illustrate Rose's model, but the fit of the models to the problems chosen for analysis is far from tight, owing to the diversity of the topics and the variety of geographic settings. The volume contains chapters by Jack Brand on local government reform in England and Sweden, Ernst Gehmacher on foreign workers in Austria, Thomas Bowden on the decline of the British-led Royal Irish constabulary, and Robert Jackson on crisis management in Canada, Britain, America and France. Hugh Heclo writes an excellent conclusion. I will discuss the chapters written by Hood, Peters and Heclo.

Christopher Hood's, "The Politics of the Biosphere: The Dynamics of Fishing Policy" focuses on fishing policy in the waters around Britain. He treats fishing policy as a dependent variable and the economics and fishing stock changes as independent variables. The latter are easily measured and constitute important variables in policy formulation. Government policy, in turn, has significant implications for fishing behavior. Hood's article demonstrates the difficulties of dealing with "free-floating" resources which cannot be contained within particular jurisdictions. He approaches fishing policy via the concepts of externalities and cycles, ratchet effects, demonstration effects and reciprocal measures. He argues convincingly that such cycles are the important determinants of changes in public policy and vice versa. Hood's article highlights the potential theoretical significance for political scientists of conducting research on comparative oceans policy.

Guy Peters', "Social Change, Political Change and Public Policy: A Test of a Model" focuses on the growth of social welfare programs in Sweden from 1865 to the present. He tests traditional hypotheses concerning the relationship between the growth and expansion of social policy and the social and political determinants thereof. He shows, through sophisticated use of time series analysis, that the determinants differ from period to period. This piece is particularly significant because it combines economic theory, hypotheses drawn from historical studies, as well as sophisticated forms of data analysis not usually used by political scientists.

The concluding chapter by Hugh Heclo is perhaps the most interesting in that he grapples with the profound issue of dynamics and statics in comparative analysis. In particular, he seeks to specify how one can approach the problem of coping with dynamic change.

In sum, this volume should be read by students of comparative policy, despite the difficulty of relating the individual chapters to the overall conceptual scheme.

TIMOTHY M. HENNESSEY

University of Rhode Island

Political Generations and Political Development. Edited by Richard J. Samuels. (Lexington, Mass.: D.C. Heath, 1977. Pp. xii + 141. $14.00.)

This book is the record, apparently verbatim, of the proceedings of the 1975–1976 Harvard-MIT Joint Seminar on Political Development. The format matters. It represents the attempt of a continuing body of specialists in political development to come to grips with the significance of generational factors in their field of specialization. Specialists in political generations attempting to come to grips with political development would have written a very different book. As the editorial introduction acknowledges, political generations and political development involve time, and their study "necessitates historical investigation" (p. 1). The policy emphasis of the contributors therefore shrinks the necessary time frame, sometimes coming perilously close to a current events discussion. This weakens the analytical significance of the book.

That significance is further reduced by the structure of the book. Ten chapters, each involving a formal presentation generally followed by oral discussion, are essentially geographical rather than analytical in theme. The geographical coverage is extensive. There are two chapters on the United States (by Samuel Huntington and Martin Kilson), and one chapter each on India (Myron Weiner), China (Lucian Pye), Israel (Amos Perlmutter), the Arab world (William Quandt), the Soviet Union and Eastern Europe together (William Griffith), and Brazil and Peru together (Alfred Stepan). There is no chapter on Western Europe, whose political development has been most intensively studied from a generational perspective since Karl Mannheim. There are few footnotes. Some chapters have good, and other chapters have skimpy, bibliographies, while still others have none. In view of the diffuseness of the discussions, the absence of an index is especially troublesome.

Conceptions of political development remain surprisingly unspecified, and seem to vary from author to author. The concept of political generation is explicitly defined, by Huntington and the editor, in three different ways. The first definition is that of Mannheim: experiences shared during formative years led to common values which do not change later in the life cycle. The second is that of the life cycle, in which values change as human beings age. The third, whose distinctiveness from the first two definitions is not sufficiently demonstrated, involves interaction among generations, in which each successive generation reacts against the values of its immediate predecessor. This last definition, judging from the case studies if not from the introductory sections, may be exemplified by conflicts between parents and children. Indeed, the most successful chapter is Myron Weiner's explanation of the succession of Motilal, Jawaharlal, Indira, and Sanjay Gandhi. The most substantial evidence for the aging hypothesis is found in the treatment of Communist ruling elites by Pye and Griffith. The most persuasive application of the Mannheimian approach to generations is presented by Alfred Stepan, who follows the careers of Brazilian military officers who served in the Second World War. It is not always clear, however, which of the three approaches is being used at any particular point in the detailed arguments. Since at least Mannheim's definition and the aging approach are mutually exclusive, this vagueness is damaging to intellectual clarity.

A more precise understanding of Mannheim seems in order. The editor sees Mannheim's generations as agents of change (p. 4), but they are themselves the product of social change. Without enough social change, there would only be one generation in politics at any given time. Later, when the editor approaches this point (p. 6), he does not sufficiently appreciate the fact that Mannheim's approach can explain stability as well as change. This insufficiency also characterizes the concluding chapter by Jorge Dominguez (p. 140). The most surprising suggestion, however, is made by Weiner and Huntington (p. vii) and Quandt (p. 84); that human beings can move from one generation to another. Karl Mannheim's body must be moving at least slightly as he attempts to engage in intergenerational mobility.

MARVIN RINTALA

Boston College

Regional Government and Political Integration in Southwest China 1949–1954: A Case Study. By Dorothy J. Solinger. (Berkeley: University of California Press, 1977. Pp. ix + 291. $17.50.)

Students of Chinese politics and history, political development, and comparative administration will all need to take note of this important contribution to their respective fields. This volume provides the first detailed study of the Chinese Southwest under early Communist rule. It is also a careful application to a well-defined set of data of some of the central ideas of the literature on political development. It is, as well, an example of the utility of Western administrative theory in organizing—and occasionally substituting for—data originating in non-Western settings. It is not surprising that such an ambitious undertaking does not succeed, totally, in any of the three areas, since the attempted leap across several intellectual chasms is too difficult as yet for any investigator to undertake. But the endeavor is nonetheless a grand one and the resulting synthesis, however partial, will provide a standard for scholars in all three fields for the period ahead.

Solinger organizes her topic first around the history of the Chinese Southwest, before 1949 and between the initiation of Communist power regionally and its gradual replacement by centralized rule from Peking in 1954. The historical framework is quickly inundated, however, by the theories and analytic categories of the developmentalists and the students of administration, so that it becomes increasingly unclear what actually happened in the Southwest after 1949. Telling history, however, is not the author's purpose; rather, it is to demonstrate the extent to which existing developmental and administrative theories assist in understanding developments in Southwest China and the extent to which such theories themselves stand up when applied to a set of data for which they were not originally designed. There is an obvious contradiction between these two goals, and the reader is consequently never entirely sure in which direction the author is heading. Nonetheless, the effort is well worth the reader's while: thanks to Solinger's work, we know measureably more about the Chinese Southwest during this period; we are in a better position to decide about the comparative efficacy of various approaches to development; and the author has clearly demonstrated that Western theories of administration can indeed be usefully applied to both non-Western/Communist political settings.

Of particular note is Solinger's success in using the products of her latter-day colleagues in China studies. This is the first such study to do so with such completeness, and it demonstrates how effective intellectual cross-fertilization can be in using the categories and results of studies of other Chinese provinces under Communist rule. She also draws a number of important distinctions: between intraregional integration and integration between center and region, between functional versus geographical integration, and between types of leaders (local whites, local reds, and outside reds). Solinger moves masterfully between inductive and deductive reasoning in her use of theory and data. After summarizing an enormous amount of factual research in a few paragraphs, she induces several tentative generalizations (which are written also with an eye toward previous theoretical work), and then applies them to several bodies of additional data not directly linked to the factual base from which the propositions were originally derived. (In her case, the original data consisted of job postings, job changes, and biographies of regional, provincial, and sub-provincial leaders; the theory was drawn from the literature on comparative administration and political development, mostly in Africa; and the additional data were culled from regional newspapers concerning policy toward trade, national minorities, and "counter-revolutionaries," three "problem areas" for the new regime.) This is the essence of inductive-deductive social scientific reasoning and its employment in this book should come to be regarded as a model of its kind.

There are problems, to be sure. Solinger contracted category-itis while writing the doctoral dissertation from which the present volume is derived. The result is a volume too filled with distinctions, lists, groupings, and concepts difficult for even the most hardened Lasswellite to weather. The book thus becomes more a handbook than a history or the testing of a single thesis. The author also takes official reports as clean fact (for instance, accepting that residual Nationalist resistance were indeed "bandits," or that those otherwise normal citizens who criticized the new regime or who happened to be from the "wrong" class background were indeed "counter-revolutionaries," or that the pre-1949 period was "pre-Liberation"). The author thus converts politics and history into mere administration, taking the spark out of the events of the period. Finally, she missed an opportunity to corroborate official documentation with other sources on events in the Chinese Southwest—Nationalist analyses and evaluations (which *can* be used with profit, albeit with the same caution that applies to Communist documents) and refugee interviews in Hong Kong, Southeast Asia, and India. While one should not expect perfection, the definitive story of this region of China under early Communist rule remains to be written.

These are the problems of success, however, and are easily put aside. On balance, this volume will remain for a long time a standard for writers in each of the three fields originally noted. It is a fine initial effort of a talented scholar in China studies and in political science as a whole.

THOMAS W. ROBINSON

National War College

Science, Technology and Society: A Cross-Disciplinary Perspective. Edited by Ina Spiegel-Rösing and Derek de Solla Price. (London and Beverly Hills: Sage Publications, 1977. Pp. 607. $29.95.)

This volume, commissioned by the International Council for Science Policy Studies, is a major effort to make accessible the considerable corpus of scholarship that has been produced during the last two decades in a number of disciplines and academic traditions. It is a literature that focuses on the social and policy dimensions of science and technology and it has long needed to be pulled together in an interdisciplinary way. The editors have done exceptionally well in assembling able people. More importantly, by wielding a firm editorial hand—for example, by providing access to each author of the work of the others in the volume—they have given us a work that is, unlike most others of its kind, exceptionally well integrated.

Part I (chapters 1–3) deals with the contextual values of science and technology in society, and gives considerable attention to the evolving critical attitudes toward these enterprises, as well as the interrelationships between the making of science policy and the scholarly effort at understanding the process itself. Ina Spiegel-Rösing, in "The Study of Science, Technology and Society," considers the positive humanistic, relativistic, de-simplifying, normative tendencies, as well as the negative tendencies of rhetoric pathos, fragmentation, lack of comparative research, and a bigness and hardness bias. Jean-Jacques Salomon, in "Science Policy Studies and the Development of Science Policy," notes the gap between decision making and science policy studies, and delineates a movement from an "age of pragmatism" to an "age of questioning." J. R. Ravetz in "Criticisms of Science," concludes that "the complex multiplicity of roles and the consequent ambiguity of self-consciousness are now essential structural features of science" (p. 85).

Part II (chapters 4–9) takes the scholarly disciplines which constitute the social study of science and technology, summarizes the key literature in each, and attempts to overcome the present fragmentation by a deliberate effort at juxtaposition with the other subfields. In "Sociology of the Scientific Research Community" M. J. Mulkay provides an account of what is currently known about the modern scientific research community. Roy MacLeod, in "Changing Perspectives in the Social History of Science," provides an analysis of the internalist/externalist debate that has raged among historians of science. In "Conditions of Technological Development" E. Layton notes that most studies of development have been done in the United States and suggests that there "is no *a priori* reason to assume that the American model in technology or science is universal" (p. 198). There are also chapters by C. Freeman on "Economics of Research and Development," R. Fisch on "Psychology of Science," and Gernot Böhme on "Models for the Development of Science."

Part III (chapters 10–15) is entitled "Science Policy Studies: The Policy Perspective," and is organized around types of issues in science policy. Though these chapters are all written by political scientists, the articulated effort to bridge interdisciplinary specialization by juxtaposing the literature and analysis in each chapter to the others in this and the previous section is successful. Sanford A. Lakoff writes on "Scientists, Technologists and Political Power." He covers, inter alia, the historical evolution, the political neutrality of science, the expanding social role of the scientist and the technologist, their political functions, and the growing issue of social responsibility. Dorothy Nelkin, in "Technology and Public Policy," focuses on "the political forces that determine priorities for the use of science and technology; that shape perceptions of their impacts and direct the means of control" (p. 430). Harvey M. Sapolsky, in "Science, Technology and Military Policy," reviews developments over the last two decades. Brigitte Schroeder-Gudehus, in "Science, Technology and Foreign Policy," discusses science and technology as power factors and notes some of the gaps and barriers in the foreign policy-making process and relates these to some of the crucial political dimensions of the international science community. Eugene B. Skolnikoff, in "Science, Technology and the International System," provides an overview of the general international effects of technology. He categorizes these as interdependence, a new meaning of warfare, emerging patterns of interactions and new actors, the gap between rich and poor nations, and the feedback between domestic foreign policy and science policy. In "Science Policy and Developing Countries" Ziauddin Sardar and Dawud G. Rosser-Owen consider the attitudes, traditions, and cultures of the developing countries and argue that "the preservation of the integrity of these must be one of the criteria of science policy." This chapter is the only one that does not measure up to the high standards of the rest of the volume, for it is marred by an ex-cathedra tone used to bolster their set of policy recommendations.

It should be noted that though this volume does not present any new scholarship or research findings, it possesses several considerable virtues. Undoubtedly it will serve for many years as the most comprehensive, synoptic listing, and analysis of the key literature now extant. Also, it provides in each of the chapters the contours of the emerging directions of scholarship and research. The authors have also included many suggestions of important research and scholarship that waits to be done, and this should be especially useful for budding young scholars looking for important terrain on which to exercise their talents. The one significant limitation of the volume is that it does not review the extensive literature from the socialist countries. The editors are cognizant of this gap, but state that their original intention to have for each chapter a related counter chapter was too difficult to achieve given their limited resources. Also, given their emphasis on the intellectual integration of the field of science and technology studies, they decided to exclude the large literature concerned with the analysis of the management of research and development.

While the volume is directed primarily at scholars in the various subfields of the emerging field, it should also be useful to teachers and in graduate courses. Last, but not least, the task of contributing toward the intellectual integration of a new field has been admirably fulfilled. The editors have set a model for future efforts of this kind.

JOSEPH HABERER

Purdue University

The Future that Doesn't Work: Social Democracy's Failures in Britain. Edited by R. Emmett Tyrrell, Jr. (Garden City, N.Y.: Doubleday, 1977. Pp. 208. $6.95, cloth.)

These essays will appeal to a variety of tastes. Those who like their conservatism raw will relish Peregrine Worsthorne's rhetorical flourishes as he appeals to the bourgeoisie to fight the class struggle more ruthlessly and thus curb the ascendant trade unions. They will appreciate Harry Schwartz's sober rehash of the standard right-wing objections to the National Health Service, a case that, as always, rests on highlighting the occasional failures and shortcomings of the system and obscuring its genuine successes. They may even swallow Colin Welch's selective and ad hominem argument that left-wing intellectuals bear a major responsibility for Britain's being "(relatively) poorer, less free, more equal, and less competitive than it was" (p. 55). Still they should find it hard to accept such outrageous assertions as Welch's claim that Beveridge was "a socialist in all but name" and that Tawney could consider a society of slaves a free society (pp. 60, 56).

Patrick Cosgrave provides a useful interpretation of the recent evolution of the Conservative party as a prelude to his recommendation that the party must break with the "collectivist thesis" of the postwar years and adhere strictly to the wisdom of Keith Joseph and Margaret Thatcher. Irving Kristol announces that "the most important political event of the twentieth century is not the crisis of political reality but the death of the socialist ideal" (p. 187). He supports this position by noting that the Soviet Union is a nasty society and by asserting that societies like Sweden and Britain must either revert to a more liberal capitalist economic order or become authoritarian. He urges businessmen and conservatives to support modest welfare statist measures to forestall socialist initiatives.

The remaining essays exert a more sophisticated appeal. Leslie Lenkowsky's essay on "Welfare in the Welfare State" might usefully have been read by the previous contributors. Lenkowsky marshals evidence that undermines their contention that rampant egalitarianism has sapped the British will to work, invest, and innovate, and thus has destroyed the economy. He does contend that the welfare state has diverted many Britons away from careers in the industrial labor force and thus weakened the economy, but this claim is questionable. People take attractive jobs where they are available and one might equally well conclude that the failure of British capitalism requires a growing public sector to offset the lethargy of the private sector. Lenkowsky's evidence actually shows that it is the welfare state that has suffered from the slow growth of the private sector and not vice versa. Lenkowsky is reluctant to draw this conclusion, but does affirm that "the programs of the welfare state deserve relatively little of the blame" for Britain's economic troubles (p. 162).

Peter Jay's "Englanditis" and Samuel Brittan's "The Economic Tensions of British Democracy" argue that there is an inherent contradiction in modern social policy; much of the power of their case stems from their reliance on Joseph Schumpeter rather than Milton Friedman. Brittan maintains that liberal democracy generates excessive expectations among individuals and interest groups that result in a disastrous and virtually incurable inflation, egalitarianism, and political overload. Jay argues similarly that "the operation of free democracy appears to force governments into positions (the commitment to full employment) that prevent them from taking steps (fiscal and monetary restraint) that are necessary to arrest the menace (accelerating inflation) that threatens to undermine the condition (stable prosperity) on which political stability and therefore liberal democracy depend" (p. 181). These powerful arguments deserve a detailed consideration which is impossible here, but it is worth noting that both writers' arguments depend on the acceptance of questionable economic premises and upon the impossibility of an effective incomes policy.

Finally, James Q. Wilson, in an essay that seems somewhat out of place in this volume offers a careful and judicious survey of crime and the legal justice in England.

Readers who expect to find the definitive account of social democracy's failure in these pages will be disappointed, for the real targets are the state socialist components of the welfare state and the more general social liberalism of modern western societies, both of which, as Cosgrave shows, British conservatism has often supported. Scandinavian social democracy, particularly the Swedish version with its emphasis on economic efficiency as a precondition for high levels of social welfare and on equality as a precondition for economic efficiency, remains largely untouched by these arguments. Nor are these essays likely to promote an enthusiastic revival of conservatism, for they have no positive vision to offer, but only a return to an inegalitarian, individualist and capitalist social order.

TIMOTHY A. TILTON

Indiana University

Gewerkschaften und Arbeitsbeziehungen in kapitalistischen Ländern. By Klaus von Beyme. (Munich: R. Piper Verlag, 1977. Pp. 381. DM 22, paper.)

This is the sixth book in as many years by Klaus von Beyme, holder of the political science chair at Heidelberg University. It is large, despite its geographically limiting focus on labor unions and labor relations in Western Europe, and covers a great variety of topics. To name but a few: union democracy and internal organization, the closed shop, socialist ideology, strikes and lockouts, codetermination and other modes of participation, incomes policies, wage-price guidelines and concerted action, and the corporate state.

But if the book is large in scope, its weaknesses are also large. It lacks a conceptual framework and a theoretical model to give order and explanation to myriad and frequently tedious facts. Von Beyme's eclectic approach, though useful to those wanting an overview, is less valuable to those who wish to explore a topic in depth, particularly since his sources are virtually all secondary. A notable gap is the lack of attention to the impact of the multinational corporation on European labor relations. If there has been an increase in transnational labor solidarity and in union political activity in Europe, then the rise of the multis is at least partially responsible.

Despite these limitations, this clearly written book does offer a considerable body of useful information. It is worthy of consideration by students of European politics and interest groups. One hopes that it will also stimulate research to fill in the analytical gaps.

CHARLES R. FOSTER

Committee on Atlantic Studies

The French Popular Front: A Legislative Analysis. By Paul Warwick. (Chicago: University of Chicago Press, 1977. Pp. 211. $16.00.)

The advent of Eurocommunism and the strong possibility of Communist participation in one or more of the governments of Latin Europe should renew interest in the lessons of the French Popular Front of 1936–1939. Four decades later, the Popular Front remains France's only experience with leftist government.

The Popular Front coalition of Communists, Socialists and radicals was hampered by ideological rifts that predated the coalition's rise to power. Confronted with the depression, the Popular Front engaged in economic policies of dubious merit (such as lowering the work week to 40 hours with, in practice, no overtime). Additional strains were brought on by labor militancy at home and by Nazi Germany and the Spanish Civil War abroad. Paul Warwick seeks to decipher the role these factors played in the Front's failure. His effort is based upon a useful synthesis of sources in political science and economics that is amplified by his own statistical analysis of parliamentary deputies, roll call votes, and 1936 election platforms.

Warwick argues that the Popular Front did not fail simply because it was inserted in an unstable and ineffective institutional and cultural framework of the Third Republic. The Third Republic had in fact dealt successfully with the conflict between bourgeois republicans and anti-republicans centered in the army and the Catholic Church. The regime was defended, however, at the price of a "stalemate" consensus on deflecting social and economic change. While the political, social, and economic organization of French society underwent relatively little change, industrialization and urbanization gradually accentuated the importance of social and economic divisions. These divisions were finally crystallized by the depression. Warwick concludes—with several nice nuances—that ideological divisions over these issues between Marxists and non-Marxists combined with stalemated political organizations to topple the Popular Front. The emphasis is thus on relatively permanent ideological splits rather than specific policy issues, such as intervention in Spain, that were to test the coalition's cohesion.

If Warwick's work leads to stimulating interpretations of the Popular Front in the small and French politics in the large, the conclusions are unfortunately drawn from empirical work that is severely flawed. In a potentially innovative research strategy, Warwick does principal components analysis (similar to factor analysis) of roll-call votes and platform statements. By showing, for example, that deputies' platform statements split similarly to roll call vote splits, he arrives at the conclusion that the Popular Front fell apart as a result of pre-existing ideological divisions.

Principal components allow the deputies to be plotted as points in Euclidean space. That is, principal components is a "black box" that "outputs" an Euclidean representation of the deputies from the "input" of their observed voting behavior.

Assume that an ideological space exists and that each deputy can be located by a most preferred point in this space. Assume further that the Yea and Nay alternatives on each parliamentary motion can also be represented as points in this space. Assume that each deputy never abstains, voting for the alternative that is least distant from a preferred point and flipping a coin in the case of a tie. Finally make some symmetry assumptions about the distribution of both deputies and motions in this space. In his important paper "A Statistical Model for Legislative Roll Call Analysis" (*Journal of Mathematical Sociology* 2:235–47), Richard J. Morrison has shown, for the above assumptions, that using the various conventional measures of association as correlation statistics will fail to recover the true positions of the deputies in a legislator-by-legislator analysis.

Warwick's work is based on an alternative approach, roll call-by-roll call correlation. This has an additional defect. Warwick shows each roll call as a single point in space whereas a roll call is in fact a pair of alternatives (lowering the work week to 40 hours has different implications when there is no existing limit to hours and when the status quo is a 42-hour week). While Morrison's legislator-by-legislator results do not pertain directly to roll call-by-roll call analysis, there is a strong suggestion that, even if Warwick were to avoid the single point problem, his Pearson correlations are inappropriate measures.

Certainly, real legislatures differ from the Morrison model. There are abstentions and many other complicating factors. But given the Morrison results, roll call analysts must now shoulder the burden of proof and relate scaling methods to a model of voting behavior in the legislature, thereby indicating that the methods can recover the positions of the deputies. Morrison's paper casts doubt on the validity of nearly all multidimensional roll call analysis that has been done in political science. As *The French Popular Front* shows, the message needs wider attention.

The flaws in Warwick's scaling are evident. Although principal components uncovers three major dimensions when all deputies are scaled together, the deputies line up on a curve in the three-dimensional plot. In fact, as Warwick acknowledges, there is basically only one dimension, the classical Left-Right dimension that closely resembles a simple Guttman scale. Interocular stress and Yule's Q coefficients uncovered this basic dimension despite the smokescreen put up by principal components. But, in the case of the critical analyses of subgroups of deputies, there are no simple patterns. We are left with the dubious principal components results.

Warwick does remark that, in any event, the analysis will be marred by strategic voting where deputies' votes do not reflect their true preferences on the motion at hand. For this reason, the study would have been enhanced by a reading of parliamentary records, journalistic accounts, and other sources that would have permitted at least an informal means of accounting for strategy. Similarly, it would have been useful to discuss the results in terms of identifiable politicians and cliques instead of showing only a nameless collection of plotted points.

HOWARD ROSENTHAL

Carnegie-Mellon University

Special Interests and Policymaking: Agricultural Policies and Politics in Britain and the United States of America, 1956–70. By Graham K. Wilson. (New York: John Wiley, 1977. Pp. 205. $15.95.)

Recent resource shortages have shown that traditional governmental policies and practices must be altered and adjusted, and innovative ways sought to deal with the new conditions of resource availability. Since our next resource crisis is apt to be a food supply crisis, it is timely to reassess basic agricultural politics and programs and to examine the political pressures which constrain policy change.

Graham K. Wilson, a British scholar, provides a provocative study of agricultural subsidies in Britain and the United States. Characterizing farm subsidies as "economically inefficient and socially undefensible, distributing benefits preponderantly to the wealthiest minority of farmers" (p. 73), Wilson examines why governmental policies therefore continue to provide farmers such largesse. His central argument is that farm subsidies are not maintained because special interests exercise inordinate power within governmental institutions; rather, agricultural subsidies persist because of governmental inertia produced by basic deficiencies in the policy-making processes of each country.

In Britain, the National Farmers' Union (NFU) is viewed by governmental decision makers and other interest groups as the authoritative spokesman for British agriculture. It maintains a close relationship with the Ministry of Agriculture, Fisheries, and Food (MAFF), and participates in the Annual Review and Determination of Guarantees which decides the appropriate yearly level for agricultural subsidies. Yet, Wilson argues, the NFU has been unable to capitalize upon the apparent political advantages its favored position presents; the MAFF has coopted the NFU, and the NFU's ability to deploy its political power is constrained by the necessity of maintaining a viable working partnership with the government. Since no major political actor, including the NFU, perceives that its role is to question policy, the British policy-making system functions without an effective means to evaluate existing policies.

In the case of the U.S., Wilson argues that the "iron triangle" explanation of interest groups, administrative agencies, and congressional committees interacting to give special interests hegemony in policy making is misleading when applied to agricultural subsidies. Competing interest groups are ideologically divided about the desirability of governmental intervention in the agricultural industry, and U.S. politicians, regarding interest groups as unreliable representatives of farmers' concerns, "do not rely on interest groups when they wish to consult an interest" (p. 99). Coalitions formed within the Democratic party through intra-party bargaining, not a bipartisan farm bloc, maintain farm subsidies. Party politics rather than clientele control also characterize actions within the executive branch. The lack of consensus has subjected farm subsidies to considerable attack and debate. Thus, unlike Britain, the basic weakness of the U.S. policy-making system is not an unwillingness to question agricultural policy, but the inability to develop the requisite leadership and political power necessary to change it.

Wilson's study is provocative because it challenges some long-established explanations of special interest influence. Yet because many of the author's arguments and conclusions beg for more comprehensive empirical evidence, the book will be disappointing to the reader who desires more systematic measurements and analyses. Some organizational problems also detract from the value of the work. For example, the book's title indicates that a definite time span, 1956–1970, was chosen for investigation. Yet nowhere within the text is it explained why those dates were chosen, and in several instances examples outside study parameters are used to illustrate points. Further, approximately two and one-half times more space is devoted to discussion of U.S. than British policies. While it is, I think, unfair to insist upon an equal treatment of U.S. and British cases, a greater degree of organizational symmetry may have been warranted.

Wilson's study does not settle debates over the amounts and types of political power special interests exercise, or over whether the British or U.S. governmental systems are more susceptible to the influence of special interest minorities. Political scientists interested in exploring these questions to understand the processes of policy formulation and change still require more convincing and systematic empirical evidence than Wilson provides. The author's contribution has been to stir up the waters again, inviting rebuttal and examination by more refined methodological approaches.

HANNA J. CORTNER

University of Arizona

Pakistan: The Long View. Edited by Lawrence Ziring, Ralph Braibanti, and W. Howard Wriggins. (Durham, N.C.: Duke University Press, 1977. Pp. vii + 485. $19.75.)

This large collection of essays on Pakistan is the product of the Wayne Ayres Wilcox Memorial Symposium held in September 1974 as a memorial to Wilcox and his contributions to the scholarly study of Pakistan. The volume includes 14 essays—13 of which were presented at the symposium and one previously unpublished essay by Wilcox.

Before dealing with the volume as a whole, I would like to comment on individual essays, since many of them are extremely interesting and add to our understanding of Pakistan affairs, past and present.

Essays which are excellent in conception and execution, and which make a substantial contribution to the field include Baxter's analysis of pre-independence Punjabi politics, Burki's essay on economic decision making, Sayeed's comparative analysis of political leadership (Jinnah, Ayub, Bhutto), and Wriggins' essay on Pakistan's foreign policy. Those essays which are also significantly helpful in explaining the problems of present-day Pakistan include Anwar Syed's essay on the Pakistan People's Party (PPP), Gilbert Brown's chapter on economic development, Lee Bean's essay on labor force structure, the essays by S. M. Burke, Norman Palmer, and William Barnds, which all deal with aspects of foreign policy/international relations, and Hafeez Malik's essay on nationalism. On the other hand, the concluding essay by Ralph Braibanti (on "research potential" in Pakistan) neither concludes the volume nor offers any interesting speculations about the future. The essay by the Pastners (on southern Baluch politics) may be a good essay, but the narrowness of the topic makes it at best tangential to a volume which is advertised to be a "comprehensive analysis" of Pakistan.

I regard these essays as excellent for a number of reasons. Craig Baxter's chapter provides valuable insights into pre-partition Punjabi politics which have had a continuing impact on politics in present-day Pakistan. S. J. Burki's essay effectively demonstrates the impact of economic decisions on political institutions and political decisions on the development of economic structures and describes Bhutto's unsuccessful attempt to restructure the government's involvement in the economic life of the nation. Khalid Sayeed's essay contributes to the comparative analysis of leadership style and the impact of leadership on political institution building. Finally, Howard Wriggins' essay helps us to understand Pakistan's foreign policy over the post-independence period, stressing both the uniqueness of the Pakistani situation as well as how foreign policy makers responded to their environments in rather predictable ways.

With few exceptions, the essays presented in this volume should be read by those interested in Pakistan and its role in South Asia. However, although this volume contributes to our knowledge of the subject and does represent the "state of the art" regarding Pakistan studies, there are several problems with this work which prohibit me from giving an unqualified endorsement. These problems are best understood in the light of the work's stated purpose and the attempts made to attain its objectives.

According to the editors, the principal purpose of this collection

> is to examine the "new" Pakistan that emerged after the Indo-Pakistan War of 1971 and the loss of East Pakistan. The authors . . . have sought to place contemporary developments in historical perspective and to suggest prospects for the future. While the book does not represent the last word on Pakistan, it is an attempt to produce a comprehensive analysis of a country whose experience in nation-building has been generally neglected by the scholarly community (p. xviii).

The introduction to the book (written by Lawrence Ziring, one of the editors) comments briefly on each essay and concludes with this statement:

> The book looks at the complex issues challenging Pakistani government and society. Each chapter has been prepared by a scholar with long experience in Pakistan. Together, they represent a distinctive inquiry into many aspects of Pakistan's national existence (p. 24).

The reader is given no more than this as a guideline for what follows and few reasons are offered why this collection is different from others. There is no attempt to deal with the theoretical, conceptual, or operational problems of studying a nation as complex as Pakistan–those contributors who choose to deal with these problems must do so individually. Since each essay could stand on its own merits, and the editing process does not provide any intellectual unity to the volume, one might rightly ask, "why publish this volume?" In other words, the individual essays are good (again, with some exceptions), but the editors can be faulted for not doing more with the material they solicited for this volume.

The collection of essays misses the target of being a "comprehensive analysis" for a number of reasons. First, there are no chapters on the important political institutions in Pakistan–the military and the civil service. Except for a few comments on the jacket cover ("Pakistan's position in the international political arena remains largely unchanged in spite of the loss of East Pakistan, but the military establishment plays a smaller role now"), analysis of the military is lacking. Likewise, the civil service, which suffered under Bhutto but has been resurrected under President and Chief Martial Law Administrator Zia, is not discussed in any detail. In fact, there is little discussion of the role of the civil service under Bhutto, and this is a weakness. Second, there is no detailed discussion of the political opposition to Bhutto, even though it was this opposition that was to force him from office (via military coup) five months after the preface to this work was written. Third–and perhaps this is the key structural problem–there is an imbalance between the coverage of international politics/foreign policy and that of internal politics. Although there is a tendency to devote attention to Pakistan's past problems in the region (relations with India which have resulted in three wars), domestic problems and the politics which either result from them or stimulated them should be the priority in any comprehensive analysis of the country. There are four essays on the international relations/foreign policy issues and only three on domestic politics and policy issues. This structural flaw coupled with the absence of any conclusion (the claim that Braibanti's essay serves this purpose is exaggerated) leads me to challenge the editors' claim of comprehensiveness.

One must admit that the task of editing symposium papers is formidable. Unfortunately, the symposium debates and the discussions of the papers were not recorded and the editors could not include these valuable commentaries in the volume. If they had been included, readers would have better understood the wide variety of opinion and lack of consensus regarding the problems and issues facing Pakistan. In the final analysis the editors are correct in maintaining that this volume reflects the state of the art and, in doing so, reveals "the neglect of the scholarly community towards a country" whose experience in national development is both similar to those of its neighbors, yet unique and requiring examination in greater detail. One hopes that further research will correct the inadequacies of the past.

ROBERT LAPORTE, JR.

Pennsylvania State University

International Politics

Revolutionary Diplomacy: Chinese Foreign Policy and the United Front Doctrine. By J. D. Armstrong. (Berkeley: University of California Press, 1977. Pp. 259. $15.75.)

This is a small jewel of a book. Except for an initial historical chapter that suffers from the tone of a doctoral dissertation, the author superbly elucidates through conceptualization and empirical testing precisely how an important aspect of communist ideology affects Chinese foreign policy. Armstrong offers two alternative models for this purpose, juxtaposing the "united front" approach with a classical alliance policy as manifest in strategic and tactical propositions that distinguish one from the other. He surveys Peking's foreign policy since 1949 to determine which periods are most appropriate for testing these models. He then applies them to four case studies: Indonesia, Pakistan, Cambodia, and Tanzania.

Armstrong operates wholly at the national actor level. At no time does he examine Chinese domestic politics for its effect on foreign policy. This is not a severe shortcoming, however, since he does not seek to explain the total basis of policy outputs but only to assess the degree of fit between one ideological component and actual behavior. He disconfirms the role of ideology in two of his four case studies, i.e., Pakistan and Cambodia, to challenge one generalization, but finds it relevant in the other two so as to focus on variables which affect decision making. He concludes that lack of strategic importance, ignorance, and geographic distance increase the likelihood that ideology will guide policy, whereas in situations close to home strategic consequences and information prompt a nonideological policy.

A potential problem lay in Armstrong's failure to utilize original language sources. Thus in discussing changes in the nuances of terminology reflecting Peking's willingness to back Indonesia, his reliance on translated materials leaves some doubt as to the equivalence of terms in Chinese and English. Fortunately, this limitation does not undermine his analysis because he is not concerned with perceptions and motivation but rather with the public manifestation of policy, primarily as communicated in Peking's own official translations. Moreover, he is extremely subtle and sophisticated in his use of these materials, weighing the implications of omission as well as emphasis by comparing such statements over time and against their counterparts from the particular country concerned. It is unlikely that reworking the same documents in Chinese would add very much or change many inferences.

A few documentary oversights deserve attention. Published State Department papers challenge the author's portrayal of both Chinese Communist and American policy from 1945 to 1949. The CCP was neither so blind to Stalin's ideological shortcomings nor so delinquent in failing to probe for an alternative relationship with the United States as Armstrong implies. An occasional suspect source dilutes the strength of his argument. There is no credibility in an Indian newspaper account of an unpublished speech allegedly made in 1968 by Yao Wen-yuan which castigates Prince Norodom Sihanouk as "a reactionary through and through" who should be overthrown. Similarly, an obscure news service report alleging gross failures in the Chinese aid program for Cambodia does not deserve inclusion in a study of this caliber.

However, these flaws are few and minor by comparison with the strengths of Armstrong's research and analysis. He usefully distinguishes between "reconciling" ideology with reality in the 1970s and "adapting" ideology to reality in earlier years. His emphasis on testing "the *specific* impact of *specific* ideological principles on *specific* policies" is exemplary for future scholarship on this question.

Even at the most mundane level of historical recapitulation, this book is a reliable and valuable guide to the events encompassed by its case studies. Armstrong is disciplined in his assessment of allegations concerning Chinese behavior, particularly covert, as weighed against evidence and logic. No political bias or preconceptions cloud his vision. His detachment is a particularly welcome corrective to some of the more tenuous interpretations of Peking's policies toward Indonesia and Tanzania, for example. In the latter instance, however, he might have included consideration of the Chinese need for copper, derived from Zambia via the highly touted Tanzam railway provided by Chinese aid.

Revolutionary Diplomacy will inform the generalist and specialist alike, and deserves wide use in the classroom and in government. It is a model for further inquiry into the wellsprings of Chinese foreign policy.

ALLEN S. WHITING

University of Michigan

The Two Koreas in East Asian Affairs. Edited by William J. Barnds. (New York: New York University Press, 1976. Pp. vii + 216. $15.00.)

Two factors combine to make the Korean peninsula a potential tinderbox. First, it is one of the most highly militarized areas in the world where over a million heavily armed soldiers of two implacably hostile regimes confront each other. Second, three of the world's great powers—the U.S., the Soviet Union, and the People's Republic of China—have mutual defense treaties with either Seoul or Pyongyang, while the fourth, Japan, has ever-strengthening economic bonds with Seoul. Should a war break out in Korea, then, it is bound to have tangible impact not only on each of the four Pacific powers but on their mutual relations as well.

It is against this ominous backdrop that the Council on Foreign Relations focused its attention on Korea in the aftermath of the stunning Communist victory in Vietnam in 1975. Out of the council's discussions grew the book under review—a collection of five articles on the various aspects of the Korean problem by five distinguished scholars.

In the introductory essay, "Old Issues in a New Context," the editor, William J. Barnds, delineates the overall situation, providing a brief historical background and underlining the criss-crossing strategic interests of the four Pacific powers in the peninsula. The second essay by Donald S. Zagoria and Young Kun Kim, entitled "North Korea and the Major Powers," examines both the contents and explanatory variables of North Korea's policy toward the four powers, particularly its two Communist neighbors and patron states. The authors show that "North Korea, like most other states in the international community, does not blindly and inflexibly pursue a given policy, but rather reacts to existing pressures and opportunities" (p. 20). A careful analysis of the various external and internal constraints on Pyongyang leads them to conclude that North Korea is unlikely to embark on an adventurous policy which may lead to war.

Next, Robert A. Scalapino probes the evolution, underlying factors, and future prospects of the relations between the two principal adversaries—North and South Korea. He shows that the breakdown of the dialogue between them is attributable primarily to the fundamental incompatibility of their strategic goals. His analysis suggests that Pyongyang has thus far been much more intransigent and inflexible than has Seoul and that, for the foreseeable future, the best one can hope for in the Korean peninsula is "peaceful coexistence" between the two halves of Korea rather than "peaceful unification." Whether "dialogue rather than conflict will prevail between the two Koreas" (p. 114), he suggests, hinges to a significant degree on the policies of the four powers.

In an essay entitled "Korea: Militarist or Unification Policies?" Gregory Henderson examines the causes and consequences of the militarization of the peninsula. In his words, "more armaments per square mile are deployed in the Korean peninsula than in any other similar arena in the world—more, even, than in Vietnam before the debacle" (p. 138). Henderson, taking issue with Scalapino's analysis, argues that the responsibility for the breakdown of the North-South dialogue must be equally shared by both sides. Nor does he concur in Scalapino's conclusion that Korean unification is all but unattainable in the foreseeable future. Arguing that "Pyongyang is not necessarily as rigid as it tends to be described and, in any case, that its position must be better probed" (p. 161), Henderson calls for an "informal four-power entente," accompanied by measures to reduce the degree of militarization in the peninsula.

In the final essay, "The United States and the Korean Peninsula," Barnds not only takes a close look at the specific contents of the American stake in Korea but also evaluates Washington's policies and options in that area. He identifies "five general interests the United States has in Korea"—(1) "the maintenance of peace in the Korean peninsula in a manner that contributes to the continuance of a balance of power in East Asia," (2) "the security of South Korea," (3) "the normalization of relations between North and South and ultimately . . . Korean unification," (4) "the development of political institutions in South Korea which provide for reasonable stability, popular participation, and respect for basic human rights," and, finally, (5) "South Korea's economic development, and the continued expansion of trade and economic relations between Korea and the United States" (p. 177). Among Washington's policy options, Barnds favors a carefully executed policy of gradual military disengagement—a policy which has subsequently been embraced by the Carter Administration. Significantly, however, he stresses the importance of using the disengagement option as a leverage over Seoul, a possibility which appears to have been discarded by Washington.

All in all, the book goes a long way toward elucidating the complex dimensions of an extremely important—and dangerous—problem in the contemporary international setting. It particularly underscores the degree to which the stakes and interests of the major powers have become not only interdependent but precariously entangled with the destinies of small powers.

B. C. KOH

University of Illinois at Chicago Circle

China and the Major Powers in East Asia. By A. Doak Barnett. (Washington, D.C.: Brookings Institution, 1977. Pp. xii + 416. $12.95, cloth; $5.95, paper.)

The premise of this book is that, for the last three decades, Chinese foreign policy has been shaped primarily by China's relations with the other major powers in East Asia: the Soviet Union, Japan, and the United States. The heart of the book, therefore, consists of three chapters analyzing China's bilateral relations with each of these three countries. In each chapter, Barnett provides a detailed history of the relationship since 1949 and analyzes the principal factors—historical-cultural, ideological, economic, geographical, and political—that have influenced it. He then discusses the issues that are likely to emerge in the years ahead and offers a brief forecast of the ways in which the relationship might evolve. An excellent concluding chapter, entitled "China and the New Four-Power Equilibrium," shifts from a bilateral focus to a multilateral perspective. Here, Barnett summarizes the political, economic, and military constraints on Chinese foreign policy, describes the quadrilateral regional system which has developed in Asia since the Vietnam War, and assesses the forces working for and against stability in the region.

While the book is broad in scope, it leaves out much. Barnett does not deal with China's relations with important nations outside East Asia, such as India and Western Europe, or with Third World countries in Africa, Latin America, or the Middle East. It is not surprising, therefore, that Barnett concludes that national security has been China's overriding goal and that military and strategic considerations have been the most important determinants of Chinese foreign policy. But it is also possible that a book with a different scope would have reached somewhat different conclusions on this point. Moreover, although the book is filled with insights relevant to American policy in Asia, Barnett offers few explicit policy prescriptions. This is largely because his recommendations for American policy toward China are contained in a companion volume, *China Policy* (Washington: Brookings, 1977).

Three of Barnett's conclusions are particularly provocative. First, Barnett argues that the most probable future for Sino-Soviet relations is neither open conflict nor a wide-ranging rapprochement nor even a continuation of the *status quo*. Instead, he suggests that both sides will eventually see the advantages of a limited detente as a way of relaxing military tensions and of gaining additional leverage over the United States. Such a detente would allow the expansion of state-to-state relations and even settlement of some of the outstanding border issues, but would still be seriously constrained by ideological differences, political disagreements, and "deep-rooted resentment and hostility" (p. 82). Significantly, Barnett believes that a limited detente between Moscow and Peking would probably be beneficial for American interests. The impact might be destabilizing in the short run, particularly if Sino-American relations had not been normalized, or if there were serious tensions between Washington and Tokyo. But these short-term difficulties "might well be outweighed by long-term benefits if a reduction in Sino-Soviet tensions were to reduce the danger of war, reinforce broader trends toward detente, and open the door to increased cooperation among the powers" (p. 87).

Second, Barnett warns against undue optimism about the future of Sino-American relations. For one thing, even after normalization, there will still be important conflicts of interest between China and the United States in such regional "conflict zones" as Korea, Southeast Asia, and Taiwan. For another, since Sino-American rapprochement has rested "more on common opposition to potential Soviet pressure or threats than shared positive goals" (p. 251), a limited Sino-Soviet detente might remove an important underpinning of Sino-American relations. And Sino-American relations will continue to be complicated by the competitive regional interests of China and Japan. As a result, Barnett predicts that the United States and China are not likely to develop "intimate relations," but will continue to be "limited adversaries" for the foreseeable future (p. 252).

Third, and most important, is Barnett's forecast of the future of the East Asian region. In Barnett's analysis, none of the four Asian powers has "overtly imperialist, territorially expansionist ambitions" (p. 304), although there are still unresolved territorial issues in Sino-Soviet, Sino-Japanese, and Russo-Japanese relations. None of the powers has the ability to establish hegemony over the others. And the balance of power in Asia is now so complex that sudden shifts of policy might well have serious unintended consequences. As a result, Barnett implicitly rejects predictions of major changes in Asia as a result of American withdrawal, Sino-Soviet rapprochements, internal Chinese developments, or Japanese rearmament. Nor does he believe that there is much chance of direct, large-scale conflict among the major powers. Instead, Barnett suggests that East Asia has two likely futures. One, the more optimistic, would see a limited Sino-Soviet detente enhancing the prospects for regional stability. The other, the less optimistic, would simply be "the continuation of competition rather than cooperation among the major powers through most of the region" (pp. 332–33), perhaps centering on the "conflict zones" of Korea and Southeast Asia.

As a work of scholarship, the Barnett volume has both strengths and weaknesses. It is based primarily on the best available secondary sources on Chinese foreign relations—so much so that the footnotes provide an excellent bibliography of Western studies of Chinese foreign policy. Barnett has supplemented these secondary sources with interviews with Chinese, Soviet, and American officials which provided new interpretations and data about such important subjects as Sino-Soviet relations under Stalin, the 1958 Quemoy crisis, the origins of the Sino-Soviet dispute, the future of Taiwan, Chinese military capabilities. Particularly praiseworthy is Barnett's insistence that China's relations with the three other Asian powers cannot be discussed solely from the Chinese point of view and that the policies and attitudes of all four nations must be given equal attention.

If the book has a weakness, it is its coverage of the domestic-political determinants of Chinese foreign policy. As Barnett would be the first to admit, he is handicapped by the lack of detailed secondary analyses of this aspect of Chinese foreign relations and must resort to impressionistic and speculative interpretations. Perhaps the publication of Barnett's book will stimulate additional research in this long-neglected area.

In short, this is an extremely important book. For the specialist, it provides an insightful overview of the current situation in East Asia with some provocative forecasts of future trends. For the lay person or the beginning student, it offers a good history of China's relations with the Soviet Union, Japan, and the United States, a fine analysis of the determinants of those relations, and an excellent description of the emerging East Asian regional system.

HARRY HARDING

Stanford University

Technology, World Politics and American Policy. By Victor Basiuk. (New York: Columbia University Press, 1977. Pp. xv + 329. $17.50.)

By its own reckoning, this book charts a course somewhere between "the stars" and "the lights of each passing ship" in analyzing the future impact of technology on international relations among developed states and especially on the United States. As such, it will appear to some to wind up in the clouds while it will appear to others to have the virtue of looking beyond the nearest shoals.

The introduction distinguishes between two roles of technology: (1) technology as a deterministic force (independent variable) that complicates and pluralizes society handicapping "decision making and mobilization of human will to steer societies effectively into better futures" (pp. 2–3), and (2) technology as an instrument (dependent variable) of volition that "immensely broadens horizons for societies to change their environments and to mold their own futures. . ." (p. 2). Part 1 then argues that, while in the past, the impact of technology was relatively deterministic and long-range, in the future its impact will be more voluntaristic and rapid, opening new "vistas for organized human will" (p. 18). What matters most therefore, is the ability of different societies to mobilize the requisite will and purpose "to reshape and restructure existing social institutions and technological superstructure to exploit opportunities presented by future technology" (p. 18).

Part 2 goes on to assess this ability within the principal advanced countries—the United States, Soviet Union, Western Europe and Japan. Japan, Basiuk argues, has exhibited the organization and will to control technology most effectively in the past but faces some loss of this ability in the future as technology progressively pluralizes Japanese society (p. 127). The Soviet Union "appears to be well prepared for taking advantage of future technology" (p. 68), although it must overcome problems of bureaucracy, low productivity and the limitations of its resources and work force (big ifs, one might add). By contrast, the United States and even more so, Western Europe are severely handicapped in their efforts to control future technology, plagued by excessive social and institutional pluralism.

Part 3 assesses the impact of technology on relations among advanced countries and concludes that the use of military power and power in general are on the decline in relations among these countries, that nonmilitary technology will be the principal source of change and destabilization in these relations in the future, and that advanced countries are most likely to react to future technological forces at the national and subsystem (regional) levels rather than the global level. "Only what cannot be solved at these two levels will be referred—reluctantly—to the global level" (p. 190).

The response of national systems, therefore, is crucial. Part 4 examines in greater detail the U.S. response. What the United States needs, according to Basiuk, is a more rational method of decision making in which "the interests of various groups would be given due consideration. . ." but "in the last analysis, the decision would be based objectively on what is good for the nation as a whole and not on the power, vociferousness, or similar attributes of a particular constituency" (p. 198). To this end, Basiuk supports the creation of a White House Science and Technology Council with authority in its field "at least as great as that of the Council of Economic Advisers (CEA) with regard to the economy. . ." (p. 211), a National Institute of Technology to systematize the identification of priorities, an adviser to the president for a Future International System to relate national policies to international interests and goals, and most importantly a national technological strategy to ensure the "comprehensive and systematic use of technology to enhance or maintain the nation's viability and vitality" (p. 215).

Such consolidation is all the more important because, as Basiuk argues in part 5, the future will bring a shift from linear, growth societies to semistationary and eventually stationary societies in which cultural and intellectual values will matter more than material ones. To remain vital in such a world, the United States must pursue the achievement of a "virtuous, enlightened society in which spiritual values flourish. . ." (p. 300). Reorienting the nation toward this goal, Basiuk concludes, "would take a president who is an inspiring and strong leader of high personal standards and attributes whom the nation—and the world—could admire and follow" (p. 311).

That Basiuk ends on such a soaring note reveals the obvious shortcomings of this sweeping study. Determining national purpose is much less an intellectual and inspirational exercise of deciding objectively what is good for the nation as a whole under the leadership of a strong president, than it is a political process of managing conflicting interests and competing national purposes. In such a process, pluralism is not only a consequence, as Basiuk contends, it is also a basic value ensuring individual choice and safeguarding against narrow and inflexible national choice. These are important strengths of pluralism, especially if, as Basiuk argues, the issues of the future will be less material (technological) than spiritual.

To Basiuk's credit, he draws our attention to these fundamental issues and reminds us that the so-called imperatives of technology may be no more than the consequences of previous and continuing political choices *which can be changed.*

HENRY R. NAU

George Washington University

The Diplomacy of Detente: The Kissinger Era. By Coral Bell. (New York: St. Martin's Press, 1977. Pp. viii + 278. $12.50.)

This appraisal of detente is more interesting than most for several reasons. One is Coral Bell's definition of detente as a strategy for managing adversary relations in a tripolar world. Rejecting a focus on only American-Soviet relations, she asserts that China is not only an integral part of detente but considering past Sino-American tensions, the latter detente is a greater achievement than the superpower one. The benefit for the U.S. is greater diplomatic leverage. Bell convincingly argues that Nixon and Kissinger were able to extricate the U.S. from Vietnam because they were able to isolate North Vietnam from Moscow and Peking. The war, which had bipolar cold war origins, could be ended because detente allowed Vietnam to be separated from the central balance.

Second, Bell highlights some of the consequences of detente for America's allies. In Australia, it allowed the government to cast off outmoded foreign policies, and in Cyprus the end of the cold war meant the end of the subordination of Greek-Turkish tensions over that island to their greater common fear of Russia and a simultaneous reduction of American influence. Interestingly, Bell does not similarly argue that detente eroded NATO bonds; quite the contrary.

Third, in an analysis of detente's critics, Bell not only reminds us of its left-wing critics who view detente as a Soviet-Chinese sell-out to U.S. imperialism but notes also the irony of the American cold war liberals' critique of Moscow while Peking's rule is far more totalitarian. She concludes that the critique is not therefore essentially an ideological one but reflects strategic concern with Russia's greater power and threat to America. Finally, and perhaps most painful to the American reader (depending on his or her perception of what the American role in the world ought to be), Bell attributes some of the claimed failures of detente—e.g., Angola—not to detente but to the paralysis of the U.S. will act after Vietnam and Watergate.

Nevertheless, while Bell finds the 1976 international situation far preferable to 1968 and concludes that the U.S. is the greater beneficiary of detente than China or Russia, she too often appears to interpret events to fit her thesis of a virtually unblemished detente. She argues that the 1973 Yom Kippur War was not only not contrary to the spirit of detente—Moscow was almost as surprised as the U.S. and Israel—but that the hostilities were ended and the subsequent disengagement agreements negotiated because of detente and Soviet restraint. This thesis has to withstand large-scale shipment of Russia's most modern arms, knowledge that Sadat having pronounced twice before that this was the "Year of Decision" could not succeed Nasser if he bluffed again, Moscow's call upon other Arab states to help Egypt and Syria fight, and support of the embargo and protest when it was called off. Was the war not ended after the cold war superpower confrontation and the postwar shuttle diplomacy unsuccessful because Egypt and the U.S. excluded Moscow?

This is not just a matter of interpretation. More fundamentally, it is a question of whether detente does not involve some rules of restraint. In Europe, Moscow interprets Helsinki and the Brezhnev Doctrine as a Western hands-off policy toward Eastern Europe; does detente not require a reciprocal attitude toward NATO countries? The issue is not, as Bell maintains, that there is little evidence of much Soviet meddling in Portugal in 1975, which is in itself disputable. Outside Europe, the question is the same. Angola, unlike the Middle East, is far beyond traditional Russian security interest. Can detente survive the extension of Soviet influence via a proxy's use of force in an area previously off-limits to superpower rivalry? Will restraint be observed only when the U.S. threatens to invoke credible sanctions, i.e., cold war tactics?

Bell also does not consider arms control central to detente. While she prefers detente with arms control, she asserts that detente could survive without it. This is a dubious contention because the fears and suspicions accompanying unregulated arms competition would spill over into the other relationships; the latter cannot be insulated when the military balance is critical for detente's survival. Bell also explains away far too easily Moscow's enormous military build-up in terms of age-old Russian defensive concerns.

Finally, Bell remains ambiguous whether Kissinger was necessary for detente or whether he was simply the right man at the right place at the right time. Nevertheless, her evaluation of his performance is refreshing, frequently insightful and thought-provoking, and a fairer one than many of his U.S. critics who see his record as either too accommodating or too outmoded in its "power politics" orientation.

JOHN SPANIER

University of Florida

Unofficial Diplomats. Edited by Maureen R. Berman and Joseph E. Johnson. (New York: Columbia University Press, 1977. Pp. viii + 268. $15.00, cloth; $5.95, paper.)

This work concerns "individuals and groups who have contact with private citizens or government officials from other countries as well as their own government" (p. 5), excepting "multinational corporations or other commercial ventures" (p. 5) who nevertheless receive subsequent passing attention (pp. 22–23). It is this "range of private international relations" (p. 5) that the editors call unofficial diplomacy.

Part 1 examines certain private channels: the Dartmouth Conferences, and the International Press Institute. Part 2 focuses on private diplomacy in international conflicts. Part 3 covers a variety of subjects under the label of "Special Approaches."

In these three sections, a reader can find useful descriptions of certain nongovernmental transactions. For example, the editors correctly recall the impact of the studies by the Club of Rome, a private, interdisciplinary group whose work widely penetrated governmental as well as non-governmental circles. The editors also provide a useful description of the contents and general nonapplication of the little-known Logan Act of 1799, passed by Congress in an attempt to regulate private American citizens in their international transactions. Berman and Johnson also make clear the wariness of governmental officials toward private diplomacy; relevant to this general theme is the case study concerning activity by Harry S. Ashmore, a journalist associated with the Center for the Study of Democratic Institutions, during the war in Vietnam.

The editors also provide a few general, analytical insights. They suggest "that the greatest chance of having a positive impact and success belongs to those initiatives that have tacit and sometimes official approval" (p. 259). They also suggest that one of the main uses of private diplomacy is to allow governments "to take informal policy soundings" (p. 260). They argue that "the effects of transnational activities appear largely indirect and long term" but that "nongovernmental groups have played a key role in raising the salience of . . . issues on the international political agenda, in offering expert advice on these issues, and in demonstrating to national and international authorities that there is public support for long-term and global approaches" (p. 263).

However, this reader was bothered by several matters. Should the idea of "unofficial diplomacy" include Lincoln Bloomfield when he conducts a seminar in Italy with participants from different nations (pp. 222–40)? Such an exercise is different in nature from the seminar conducted by Phillips Talbot for the explicit purpose of helping to resolve the international conflict centering on Cyprus (pp. 159–67). Especially in part 3, the concept of unofficial diplomacy is so broad and amorphous that it could include *any* transnational activity. This raises the question as to whether the organizing concept is so all-encompassing that it lacks meaning, or lacks meaning *different* from other concepts such as transnational relations.

Furthermore, the editors opt for description by practitioners. This does give the reader a grass-roots view of the transactions studied. But there are problems. We get a view of some of the trees but we lack a view of other trees and of the forest. Perhaps the most interesting chapter concerns how the Commission of the Churches on International Affiars helped resolve the internal war in the Sudan in 1972. But the description is not analytical. Why did the CCIA succeed whereas others had not? What resources did the CCIA bring to bear? Was this simply historical accident without broader significance? Other authors of chapters give extensive and detailed description, only to come up with meager analysis: the Quakers in the Middle East were able to supplement official communication (p. 86).

In summary, *Unofficial Diplomats* is a disappointing book on a very important subject. Readers can certainly increase their knowledge of descriptive information by reading this work, and there are a few analytical insights. But the contributions of the various authors are uneven in quality, and the editors themselves do not provide the conceptually clear and systematic overview of transnational, non-governmental activity found in other analytical works, such as *Transnational Relations and World Politics* edited by Robert O. Keohane and Joseph S. Nye.

DAVID P. FORSYTHE

University of Nebraska, Lincoln

Regimes for the Ocean, Outer Space, and Weather. By Seyom Brown, Nina W. Cornell, Larry L. Fabian, Edith Brown Weiss. (Washington, D.C.: The Brookings Institution, 1977. Pp. xi + 257. $9.95, cloth; $3.95, paper.)

Although "intended to be primarily a layman's introduction to the problems of managing the earth's commonly used resources" (p. 1), this is an important book for scholars and decision makers as well. It is well written, thoughtful and provides a useful framework for examining the management of areas and resources hitherto beyond the reach of human use and national jurisdiction. The authors limit themselves to three "commons": the ocean, outer space, and the weather and climate. They find that, "without substantial alteration of the traditional framework for using these international 'commons,' competition will progressively embitter international relations—especially between the technologically advanced and the technologically lagging countries—and result in waste and degradation of national resources" (p. vii, foreword by Bruce K. MacLaury).

With respect to the ocean, the authors' warning appears to have been heeded neither by the U.S. nor by the technologically lagging countries. As the U.S. government conducted a "review" of its participation in the Third U.S. Conference on the Law of the Sea between the Sixth and Seventh Sessions, strong lobbies urged Congress and important executive departments to accept legislation that would, in effect, assign property rights by unilateral action to valuable mineral resources beyond national jurisdiction, thereby eliminating their status as global "commons." The minerals regime to be provided would be free-enterprise and open-access. Evidently, the price mechanism would be expected to reconcile supply with demand, protect the global ecosystem, and provide a dependable international dispute settlement system. In this way, the U.S. sought to avoid what it considered to be a dangerous precedent in international management, a provision in a draft treaty for a strong International Seabed Authority to regulate the mining of seabed minerals. It may also have sought to avoid an unwelcome dependence on imported minerals needed for the steel industry.

This ideological rigidity has been matched by the Group of Seventy-Seven in support of the New International Economic Order. Instead of accepting a U.S. plan in 1970 that would have limited national jurisdiction to the shore side of the 200 meter isobath, leaving the rest of the ocean more or less as the "Common Heritage of Mankind," the members opted for exclusive national economic zones, extending at least 200 miles from shore, and broadened territorial seas to ensure national control over their coastal zone resources. This experience may be repeated in outer space and weather regimes. While the authors agree that national governments are still better qualified than any other public authority to ensure that users, i.e., special interests, act in accord with broader public interests, both national and international, they warn that the nationalistic response may "be basically retrogressive" (p. 16). With respect to the three "nonland realms" they consider, there are dangerous contradictions between the world of growing material interdependence and the political world of exclusive sovereignties.

Chapters 1 and 19 deal generally with the problems of international "commons." The former deals with the natural characteristics of the three realms that "elude the kinds of local and national regulations applied to commonly used resources on land" (p. 3). The reader learns, or is reminded of, the problems of ubiquity, scarcity, indivisibility, and externality. They then consider traditional regimes and alternative future regimes for these resources or realms. The latter chapter closes the study with six general propositions and four principles. Although each resource regime presents common issues of international accountability, efficiency versus equity, and conflict resolution, accommodation of these issues requires policies and institutions unique to each sector.

The remaining chapters deal with the specific problems of administering the realms of ocean, space, and climate. Given the number of usages involved in each realm, the treatment is necessarily broad-gauged and precludes analytical depth in the treatment of specific problems such as off-shore oil and gas exploration, remote sensing of the earth from outer space, or rainmaking and typhoon modification. Breadth, however, does not in this case mean superficiality. A major challenge today is to understand the problems of managing what the authors call "shared environments." Without such understanding, the interacting parts of these environments will be managed for the benefit of special industrial interests in particular nations. While the sum of the parts could theoretically add up to a harmonious whole, such a result appears doubtful.

DANIEL CHEEVER

University of Pittsburgh

Arms Control and Technological Innovation. Edited by David Carlton and Carlo Schaerf. (New York and Toronto: Halsted Press, John Wiley, 1977. Pp. 366. $22.50.)

This book is a collection of 21 papers presented in 1976 at the Sixth Course of the International School on Disarmament and Research on Conflicts held at the Collegio Missionari Verbiti in Nemi, Italy. It is unclear when the first five courses were held or whether they resulted in similar published editions, but the proceedings appear to be very stimulating and worth preserving.

In addition to the 21 contributing authors, approximately 50 others attended this two-week seminar. The participants represented a cross section of international scholars interested in the interaction between the imperative of military security and the desire for some measure of arms control. Considering the major financial backers of the enterprise (the government of Italy and the Ford Foundation), it is not surprising that the majority of participants were from Western-alliance states, including 17 Americans and 12 Italians. However, seven participants came from five East European countries; four delegates from Austria and three from Finland represented neutral Europe; and a total of nine delegates came from Third World states as far afield as Bangladesh, Mexico, and Chad.

Representing diverse backgrounds, the participants ranged from graduate students to professors in fields as varied as economics, political science, industrial engineering, and astronomy. There were also practitioners from national ministries of defense, foreign affairs, and atomic energy, as well as representatives from such extra-governmental organizations as the European Economic Community and the Pugwash Conferences on Science and World Affairs. About 25 were college professors of international relations.

The two editors, seminar director Carlo Schaerf, a physics professor, and David Carlton, an historian, do not arrange the 21 essays in any coherent or thematic order. To Herbert Levine, a political science professor, fell the unenviable task of writing a 25-page introduction entitled "Summary of Proceedings." He tries to impose a three-tiered framework (dealing with the causes, the consequences, and the control, of the arms race) on what follows, but does not quite succeed. First of all, neither the rest of the book, nor any individual essay, conforms to the "causes, consequences, controls" model; secondly, perhaps for diplomatic reasons, none of the essays is specifically mentioned by name or author so that much of the introduction reads like one of those compromised United Nations reports where everyone's point of view is included, but no judgments as to quality are made.

Despite the absence of any overall framework, there are some excellent individual efforts. The most intellectually provocative essays are the final three. Istvan Kende, an Hungarian professor of international relations, has a piece entitled "116 Wars in 30 Years." In addition to an interesting list and several graphs describing conflicts between 1945 and 1974, Kende reveals that the 116 wars involved 69 countries, resulted in 25 million deaths, and represented about 350 total years of war; there were only 26 days in the 30-year period when there was no such strife somewhere in the world. Among Kende's conclusions were that 80 percent of the wartime was devoted to anti-regime (as opposed to tribal or border) wars, and that most conflicts attracted foreign military intervention which made them longer and more intensive.

A stimulating paper by Mary Kaldor is entitled "The Role of Arms in Capitalist Economies: The Process of Overdevelopment and Underdevelopment." The first half of her thesis strives to disprove the myth that military spending is economically beneficial by citing in statistical detail the deterioration of the American economy since 1945. Then Kaldor analyzes the relationship between military spending and the economy in underdeveloped states, and offers a series of counterexamples to disprove three general theories (the orthodox, the modernizing, and the Marxist ones) about arms expenditures and development.

The most ambitious article of the book is "Economic and Technological Prerequisites for Achieving Political and Military Stability," by Tom Stonier, an American biologist and peace researcher. In an effort more fascinating and comprehensive than anything on the subject since Zbigniew Brzezinski's *Between Two Ages*, Stonier takes the macro-historical approach and posits that the world has moved from an agrarian to a mechanical to a communicative era. The technological innovation of the communicative era, he feels, is laying the foundation for the demise of war as a social institution. The arms race is but a reflection of cultural lag and will end when the world moves more totally into this new and final stage.

While many participants disagreed with some of the specifics of these three essays, they were by far the most conceptually innovative and intriguing in the book. The remaining 17 papers range in style and substance from the theoretically elegant and well wrought (such as Enid Schoettle's on security policies and nonproliferation) to the reactions of such armchair propagandists of particular points of view as Harvard's Michael Nacht and the Soviet Academy's Michail Milstein (on SALT and detente). In between these extremes are some particularly good case studies of specific topics such as the United States' sea-based deterrent (by Australian Peter King), the cruise missile (by American NATO officer Robert A. Nalewajek), nuclear weapons testing (by the Arms Control Association's Thomas Halsted), and a theory of "defensive deterrence" (by the Netherlands' Bert Roling).

In addition to Schoettle's comprehensive treatment, there are four other studies of aspects of the nuclear proliferation problem: by editor Carlton on the Anglo-American nuclear relationship; by Bowyer Bell on sub-state actors; by Mario 'n Mushkat on the Middle East; and by Steven Baker on the implications of peaceful power reactors in threshhold nuclear-weapon states.

The arms control point of view is competently presented in a pair of essays by Herbert Scoville and William Epstein, retired civil servants of, respectively, the U.S. and UN disarmament establishments. Hans Morgenthau did not attend, but dusted off and submitted by mail his now-classic musings on the distinctions between nuclear and conventional weapons.

Finally, three authors address themselves explicitly to the connection between arms control and technological innovation, the title of the book. They are: legendary U.S. presidential advisor Herbert York who discusses reconnaissance satellites; Kosta Tsipis who waxes technical on precision-guided missiles (PGMs); and Jorma Miettienen who covers not only PGMs, but also RPVs, AWACs, HOTs, TOWs, Nd-YAG lasers, and a host of other innovations with relevance for tactical nuclear war (TNW) in Europe. These last two articles in particular are so unintelligible to the general reader as to justify the book's title. Because the other 17 essays diverged in such interesting directions, however, the result is a potpourri of articles with something for almost everyone interested in serious work on the several subjects cited.

MICHAEL J. SULLIVAN III

Drexel University

Public Papers of the Secretaries-General of the United Nations, Vol. 7, 1965–1967, U Thant. Edited by Andrew W. Cordier and Max Harrelson. (New York: Columbia University Press, 1976. Pp. xvii + 633. $27.50.)

The period covered by this selection of U Thant's papers was a tumultuous one. The dominant theme was war—in Vietnam the intensification of violence; in the Middle East the Six Days War following the withdrawal of the United Nations Emergency Force by the Secretary-General; civil war in the Dominican Republic and massive intervention by military forces of the United States; the resumption of hostilities between India and Pakistan in the Rann of Cutch and Kashmir and at other points along their common border in the west; and the precarious position of the United Nations force in Cyprus in the face of threatened renewal of international hostilities in that troubled island. Toward the end of the period, the Secretary-General bemoaned the fact that the "international political situation has not only not improved; it has in fact deteriorated considerably."

It was the time also when the UN General Assembly was victimized, nearly immobilized, by great power controversy over the financing of peacekeeping. Behind that lay the struggle of the titans over global ascendancy. In the United Nations argument over the implementation of Article 19 of the Charter, the Soviet Union demonstrated that it was able to deny the United States the goal it sought. In United Nations terms that was important. The Secretary-General is recorded more than once in these pages as regretting the result that the financing of future peacekeeping could be only voluntary, as exemplified by the Cyprus precedent. More significant was the fact that the issue in the UN, and its outcome, accurately reflected—indeed, it symbolized—the shifting balance of global power and the attendant changes in the capacities for world leadership.

The documents in this volume, comprising selected state papers, speeches and press conferences of the Secretary-General, amply represent U Thant's frustration over the situation so well symbolized by the Article 19 controversy. The record of his impotence in the face of the principal parties' commitment to the Vietnam War is here. So is his embattled defense of his decision to withdraw UNEF in response to Egypt's request that it be withdrawn. These pages record how reluctant U Thant was in the circumstances to be elected to a second term as Secretary-General. It is not surprising that the tone of his message was gloomy and his prognosis pessimistic when he addressed the meeting in San Francisco to commemorate the twentieth anniversary of the United Nations. As he made clear on that occasion, he found it difficult to reconcile his somewhat ascetic idealism with a world of power politics: "The concept of power politics, whether as the instrument of nationalism or of ideological extremism, is the natural enemy of international order as envisaged in the Charter." Some at least of those who had participated in the founding conference 20 years earlier might not have agreed that the "international order" of the charter was intended to be that remote from the world of power politics.

U Thant's perception may have been faulty and his pessimism excessive. Detente did not succumb to Vietnam. Great power rivalry has not yet led to Armageddon nor has it prevented the apparent emergence of a will to peace among some at least of the primary parties in the Middle East. U Thant's impatience with the great powers is understandable for a native of a small power strongly moved by Buddhist ideals. It is not clear that his outlook ideally suited him to optimize the effective leadership of his office, even within the narrow limits of what was feasible.

The period covered by these papers was a time of other emergent themes. Although the editors' selection emphasized the issues of war and peace, there are glimmerings also of the new agendas which have come to predominate in the affairs of a United Nations denied a more central role with respect to the old ones. There are reflections in these papers of U Thant's passionate concern for poverty and inequity in the world, his understanding of the relevance of population issues and his enthusiasm for the beginnings of UN programs in this realm, and his sympathetic support for the mobilization of effective forces to alter the balance of racial power in southern Africa. The United Nations Development Programme was formed during this period, out of the Expanded Programme of Technical Assistance and the UN Special Fund which had preceded it, and U Thant correctly heralded the significance of this change. The three introductions to his annual reports chronicle the emergence of UNCTAD and UNIDO as instruments for projecting the goals of developing countries in their efforts to attract support and sympathetic policies by the developed ones.

Altogether, the selections give a well-balanced picture of U Thant's utterances. The editors have provided succinct and very helpful summaries of the main issues.

LAWRENCE S. FINKELSTEIN

Northern Illinois University

The Dynamics of Detente: How to End the Arms Race. By Arthur Macy Cox. (New York: W.W. Norton, 1976. Pp. 256. $8.95.)

This book claims to be a road map for ending the arms race. It covers the broad scope of U.S.-Soviet relations, including SALT, Europe, economic factors and the clandestine battle between the CIA and the KGB. Unfortunately, the book contains far more wishful thinking than realistic blueprints. The arms race and cold war are deeply rooted aspects of the U.S.-Soviet relationship. They will not be altered easily and certainly not by the simplistic prescriptions suggested by Arthur Macy Cox.

This book has a legion of deficiencies. But space only permits a limited catalog of its errors of fact and questionable analysis. In claiming that the Soviet Backfire bomber was raised by the U.S. in SALT-II as a ploy and not a serious issue, Cox states: "The Backfire was not even mentioned prior to the Vladivostok talks because it was generally considered to be a medium-range bomber designed to attack targets in Western Europe and Asia, as well as shipping near the Soviet Union, according to civilian and military officials in the Pentagon" (p. 92). In fact, the Backfire was a major issue within the U.S. bureaucracy during SALT-I. It was repeatedly considered in SALT proposals and there were sharp disagreements between civilian and military intelligence analysts over the range and capability of the Backfire and its potential against the U.S. In fact, for several years it was one of the two or three most heated issues in the annual intelligence estimate of Soviet forces. It was also raised with the Soviets a number of times during the SALT-I negotiations, but was not made a major issue by the U.S. since bombers were set aside during SALT-I and not included in the limitations.

In a section arguing for an ambitious SALT-II agreement, Cox states that if the U.S. gave up on its goal of strategic superiority and agreed to parity with the Soviets, savings of $25 billion per year would be possible (p. 96). In fact, the total direct cost of our strategic forces is only $15 billion and if support and training costs are allocated, the total only rises to $25 billion. Thus, it is difficult to envisage the savings Cox suggests unless both sides were to fully phase out their strategic forces.

In his assessment of Soviet SALT goals, Cox states that General Secretary Brezhnev and the Soviet leadership are willing to negotiate significant limits on strategic forces and reductions in numbers of weapons if only the U.S. were willing (p. 81). Yet when President Carter made a proposal of this nature in the spring of 1977, it was turned down flatly by the Soviet leadership and attacked bitterly by Soviet Foreign Minister Gromyko. There are many other similar assertions regarding Soviet willingness to agree to significant arms control measures that would likely suffer a similar response if pushed strongly by U.S. leaders.

In a section discussing the need for a valid U.S. strategic concept, Cox badly distorts the changes to strategic policy advocated by former Secretary of Defense Schlesinger, calling it a "counterforce" doctrine. While Schlesinger called for a modest amount of "hard target" capability for use in certain limited situations, he did not advocate building up the type of preemptive, first-strike capability that Cox implies was the heart of his strategy (pp. 85–89).

The main thrust of the book (and about half of its material) is related to strategic forces and SALT. But Cox also covers conventional arms competition, clandestine operations (the best section of the book), and the economics of detente in the U.S. There are occasional useful parts, but the book suffers from a lack of realism that seriously undermines its conclusions and recommendations.

PHILIP A. ODEEN

Coopers and Lybrand

Interpreters for Nigeria: The Third World and International Public Relations. By Morris Davis. (Urbana: University of Illinois Press, 1977. Pp. 197. $10.00.)

This book reviews the activities of nine public relations firms hired not by the Third World but by component elements of the Federation of Nigeria. Of the six substantive chapters, one presents firms employed by the eastern and western regions and the federal government between 1957 and 1964; another does the same for the period 1966–67; and two chapters each review firms employed by the federal military and the Biafran governments. Much of the book covers minutiae, but it deserves careful attention for what it illuminates and suggests.

The style of the hired professionals varied according to client needs and preferences. Biafra, whose aim was to change government policy through popular outcries, opted for a "brash" approach (a bumper sticker distributed in Britain read "Put a Dead Biafran in Your Tank," referring to oil companies which were indirectly financing the war). The federal government, whose targets were government and other elites whose policy it supported, chose the "decorous" approach of low-key communications. Target audiences in both cases ranged from legislators to officials of the executive branch, private and special interest groups, general public opinion and, of course, the news media.

The present Nigerian government's antipathy toward the western press, and an explanation for the more widespread call for "development journalism," may be illustrated by events which occurred during the civil war. General Gowon, for example, gave a two-hour TV interview during which he pleaded mostly that Biafra consider negotiations and peace; when asked what would happen if Biafra were to persist in fighting, he raised a fist and declared "we will crush them." That brief episode was all that was shown to the British public. African predilections for controlling and slanting the content of foreign news reports must be understood partially in this context, although public relations agencies are not useful channels for influencing the news media, since they tend to be suspect in the eyes of the press.

Both Biafra and Lagos were clearly guided by realpolitik in the instrumental use of public relations firms. The firms became additional weapons in the struggle, and hence served to exacerbate the conflict. Moreover, neither side in Nigeria had a working familiarity with the target countries, and the agencies they hired knew very little about Nigeria or Africa. Ignorance, the personal basis for agency selection by African officials, and the degree of commitment which agencies tend to develop for their clients' cause led to many employer/agency frictions over the style and content of public relations, unpaid bills, and occasionally political strategy. Problems arose because agencies insisted that they advise rather than flatter, and employers insisted that they be on tap rather than on top; Davis suggests that such problems are common, inherent in the situation and not susceptible to easy solution. Nonetheless, hired agencies should bear in mind that the effective use of funds and effort is very limited. These limits are not easy to discern; they relate to the target countries' nationalism, to a feeling that too much is resented as illegitimate intervention in domestic affairs. Consequently Third World countries hiring foreign public relations agencies should keep their efforts modest, and seek to identify with existing social and political trends and interests in the host country. They must appear to sensitize and support, but not to supplant, domestic structures in target states.

The biggest mistake a client can make is to assume that, in a democratic target state, foreign policy is decided democratically. British public opinion and mass media were unquestionably pro-Biafran. Yet the government and party leadership on both sides of the aisle remained unresponsive. Consequently the public relations efforts of both Nigeria and Biafra had no measurable impact on British or, for that matter, on American, policy. Biafra seemed not to understand that even in a democratic polity an aroused public reinforced by special interest groups may alter government policy only if a foreign policy issue divides the country significantly, or if a major party, for reasons of its own, appropriates an issue and injests it into the decision-making process. This was not the case with the Biafra-Nigeria conflict, which competed for attention with other weighty international events.

W. A. E. SKURNIK

University of Colorado

Nuclear Power: Development and Management of a Technology. By Frank G. Dawson. (Seattle: University of Washington Press, 1976. Pp. xii + 320. $20.00.)

Frank Dawson has written an interesting book about the development and management of nuclear power. His approach is historical, focusing on the evolution of a technology delivery system. This system may be regarded as the institutional infrastructure which makes possible the research and development, deployment, and use of a new technology.

Dawson recounts how atomic energy emerged from the wartime effort to construct an atomic bomb. He shows how an erstwhile civilian agency, the Atomic Energy Commission, spent its early years helping the nation fight a cold war by supplying nuclear deterrents to military customers. He describes the transition of the atomic program in the 1950s. In that decade, the development and introduction of the peaceful atom became a national goal. He recalls the many carrots and sticks it took to get the user industry, the electric utilities, to accept atomic energy. Finally, he shows how when atomic energy finally did arrive as a competitive energy source in the middle 1960s it ran into opposition from environmentalists and other groups which had become increasingly prominent.

While the information in this book is familiar to students of science and technology policy, it is well presented and serves as a welcome reminder of how short-sighted public policy can be. There were many friends and very few critics of the peaceful atom in the early days. It was easy to focus on the immediate problem, which was to get the technology launched, and to postpone consideration of the consequences of radioactive waste disposal, the plutonium economy, proliferation, and other nuclear matters that bedevil us today. The most striking finding, Dawson reports, is how late the opposition and criticism came. The atom was well along the cycle of commercial introduction before such criticism began to slow down the process of deployment.

The primary reason for this, Dawson indicates, is the subsystem of relations that gradually evolved between the Atomic Energy Commission, the Joint Committee on Atomic Energy, and the nuclear industry. There was conflict within the subsystem, to be sure, particularly over the pace and direction of reactor development; but, where outsiders were involved, the subsystem was generally united. Not until the break-up of the subsystem in the 1970s did critics (and non-nuclear technologies) have what they regarded as a fair hearing in the halls of government.

Again, the notion of a policy subsystem is familiar, but it is nevertheless important. At a time when the Department of Energy is considering how to develop and deploy numerous non-nuclear technologies such as solar, geothermal, and the like, it might try to learn some lessons from the experience of the peaceful atom. Perhaps the most significant lesson is that an efficient technology delivery system depends upon the power behind it. It took years to build the power of the atomic energy subsystem. Once in place, that subsystem performed efficiently according to its view of the public interest. The problem, of course, is that the public interest changed in the 1960s and the subsystem did not. The lesson seems to be that efficiency in a technology delivery system is not enough. There must be better responsiveness and adaptation to changes in the environment.

Dawson seems to give the atomic technology delivery system high marks. However, while the peaceful atom is in place and likely to diffuse still further, the controversies surrounding it suggest that all was not well with past management of the technology. An analysis less oriented toward efficiency and more toward admiministrative responsiveness might not have reached as favorable an evaluation.

Overall, Dawson has written a good book. It will provide case material for students and practitioners of science and technology policy. We need to probe the meaning of America's experience with the peaceful atom. Is it a success? a failure? or something of both? Why? Could we have done better? How? What values lie behind our judgments? To the extent Dawson helps us to ask these questions and stimulates us by his own answers, he has written a book well worth reading.

W. Henry Lambright

Syracuse University

Problems of World Modeling: Political and Social Implications. Edited by Karl W. Deutsch, Bruno Fritsch, Helio Jaguaribe, and Andrei S. Markovits. (Cambridge, Mass.: Ballinger Publishing Company, 1977. Pp. v + 423. $17.50.)

This volume represents a collection of articles and commentaries resulting from a conference at Harvard held in 1976. The conference task, in Deutsch's words, was "to make a contribution to an ongoing chain of activities, to developing simulation models of major, general trends in world economics, world politics and world ecology" (p. 5).

Humanistic behavioral scientists with an interest in global modeling and computer simulation will find the volume rich with collective self-reflections on theory and method. Other scholars, who see a close fit between theory and practice and who seek to enrich both in the context of a commitment to fundamental social change, will judge the volume harshly on the basis of sociology of knowledge, theoretical, methodological, and practical grounds.

From the standpoint of behavioral science criteria, this collection usefully addresses a number of issues relating to global modeling. First, the most influential world models are critically examined. Hayward R. Alker and Ann Tickner review and critique the models of Forrester (*World Dynamics*), Meadows et al. (*The Limits to Growth*), and Mesarovic and Pestel (*Mankind at the Turning Point*). Carlos Mallmann describes the Bariloche Model (*Catastrophe or New Society?*). William D. Nordhaus evaluates these models in the context of a call for modeling policy and policy-making variables.

Second, several essays deal with the problems of data gathering and use. Richard Stone discusses accounting frameworks as they relate to economics, the environment and what he calls "social demographic data." Data-related issues are raised in response to the Stone article by Walter Isard, Anatol Rapoport, Otto Eckstein, and Alker and in articles by Bruce Russett and Charles Lewis Taylor. An example of the use of mail flow data is included by Richard L. Merritt and Caleb M. Clark.

Third, statements throughout the collection raise issues about relationships between research and policy—as, for example, Deutsch's call for the construction of "steering models" that would facilitate rational intervention in the policy process. Fourth, three long essays explicating computer simulations in conjunction with historical and quantitative analyses of subsystems of the globe illustrate the need for disaggregated and more complex modeling efforts. James P. Bennett and Alker report on a computer simulation of the "Southern Pacific subsystem" and Chilean policy making in the nineteenth century. Stuart A. Bremer and Michael Mihalka investigate the stability-generating proclivities of balance of power theory as reflected by writers who view balancing as an "automatic process." P. N. Rastogi provides a complex model of "societal development" and examines its validity for Brazil, India, Nigeria, and the United States. Fifth, Saul Mendlovitz comments on the work of the conference from the perspective of the World Order Models Project which sees the world from the vantage point of "interpenetration," or the interconnectedness of human processes. On the other hand, issues raised by Dieter Senghaas and Fernando Henrique Cardoso suggest a number of fundamental criticisms that scholars committed to social change might level at the way in which global models treat theory, method, and action.

First, critics will question the non-participatory aspects of world modeling efforts. Implicit in the conference is a normative commitment to rational policy at national, regional, and global levels. Manfred Kochen calls for "the development of models in a direction that will make them actually be used by planners, resulting in wiser planning" (p. 380). The conference participants never raise issues about the kinds of interests and values that motivate modelers and policy makers in their activities nor questions about why "wiser planning" has not existed before these modeling efforts. One might presume that the existence of expertise or the lack of it is a critical variable in explaining recent world history. This view ignores the possibility that elite interests will determine model acceptability and that the interests of masses of workers and peasants around the world may not be served by the fusion of expertise and policy.

Second, critics might reject the presupposition that progressive social change can emerge from the data lab, board room, executive office, or international organizational headquarters, and might assert that sophisticated theory and practice requires analyzing or engaging in the struggles of masses of the world's citizens. These critics might suggest that committed scholars cannot help but see the world differently in the academy from how they see it in the community, the workplace, the streets, or the rural countrysides. Senghaas alludes to this point when he asks why modelers have not considered the experiences of China, Cuba, North Korea, and Albania in their modeling exercises.

Third, Cardoso raises a methodological challenge to "the positivist assumption" from the standpoint of dialectics. He asks whether the complexities and contradictions embedded in historical processes can be uncovered by conventional social science techniques and more sophisticated modeling techniques. In his words: "We are considering that at some moments economic process is transformed into political process. But these processes are transformed by the interior, and not transformed because of the addition of another dimension or quality" (p. 141).

Fourth, critics might suggest that existant models are ahistorical in conception and method. Through the selection of key variables and longitudinal analyses, modelers require simplifying assumptions about economic change and political struggles over time. This, some will argue, necessitates the flattening out and indeed falsification of history.

Fifth, critics will argue that most of the contributions to this volume, with the exceptions of Senghaas, Cardoso, Russett, Jaguaribe, and to a lesser degree Alker and Mendlovitz, are insensitive to recent theoretical and empirical work in radical political economy, dependency, and Marxist perspectives. Occasional references to Marxist theory suggest an unwillingness to reflect seriously on its possible utility for modeling efforts as in Deutsch's reference to the philosophies of Marx and Hegel "which assumed that in every complex situation there was one, and preferably only one, master link that a philosopher of sufficient insight, like Hegel or Marx, or later Lenin, could identify" (p. 8). Finally, as Russett suggests, most modeling efforts have not given adequate treatment to transnational actors.

In sum, the quality of this symposium will be seen differently by those sympathetic to the modeling tradition as opposed to those, like me, who see more theoretical and practical value in a modern political economy perspective influenced by the Marxist tradition.

HARRY R. TARG

Purdue University

The Politics of Attraction: Four Middle Powers and the United States. By Annette Baker Fox. (New York: Columbia University Press, 1977. Pp. viii + 371. $15.00.)

This book is clearly intended to be a contribution to the growing literature in international politics which, in contrast to the pervasive power politics paradigm, emphasizes the influence of lesser powers. Whether or not this particular book overstates the case—a question considered below—the author is pioneering a much needed approach to the study of comparative international politics and foreign policy.

Annette Baker Fox has provided an example to those who find the common, single-country case study too atheoretical and idiosyncratic, and the multi-case, quantitative study too generalized and abstract. She compares the bilateral relationships between the United States and Canada, Mexico, Brazil and Australia across a variety of issue areas (defense, trade, investment, "global foreign policy," environment, immigration, etc.) over the postwar period. She has visited each of the four middle powers, has digested masses of information—albeit largely from secondary sources—and has produced an informative survey of the four relationships. The analytical approach adopted is a useful corrective to the lack of substance in generalized studies and the lack of systematic comparison in single-country studies. The sort of comparison Fox provides is probably needed in all four of the cases she deals with, but it is unquestionably needed in her major focus (and the case with which I am most familiar)—the Canadian-American one. The demands of, and the difficulties inherent in, the comparative case-study approach are considerable, however, and some readers may find that *The Politics of Attraction* does not successfully meet these challenges.

The actual errors of fact seem few enough, although there are references to the Canadian "Secretary" (rather than Minister) of National Defense (rather than Defence), to a nonexistent Canadian "Treasury" ministry (when the Department of Finance was probably intended), to the U.S. having recognized China, and so on. Of much greater import is the disturbing frequency of generalizations which seem either superficial, or suspect, or both. Thus, for example, it is stated that Canada and the other four hold the U.S. in "awe" (p. 2), that Prime Minister Diefenbaker "strove for independence" (p. 13), that the Canadian Cabinet "plays an important role in foreign policy" (p. 14), that Canada-U.S. wartime collaboration was "easy" (p. 287), and involved only "slight" risks (p. 267), that there is a "constant" domestic pressure in Canada for "independence" (p. 291), and that the Canadian government has a "distaste" not only for formal bilateral organizations but for "agreements" as well (p. 291). Many if not most students of Canadian-American relations would find many if not most of these bald assertions at least debatable and some extremely dubious. Such generalizations perhaps arise from a failure to distinguish between policy attempts and achievements and between policy rhetoric and action.

Moreover, too few of the book's generalizations appear to have been confronted with available evidence. At one point, for example, the Trudeau government's decision to reduce Canadian NATO forces is apparently attributed—without elaboration—solely to "shifting domestic pressures" (p. 269), whereas both the major existing study of this decision and public opinion polls at the time suggest little real pressure existed. At the same time, when quantitative data are presented they often appear incomplete or less than appropriate. For example, military expenditure and capital flow figures covering only the late 1960s—early 1970s period are used in discussions of "defense" collaboration and investment over the entire postwar period, while figures on GNP growth rates (rather than total GNP) are employed to demonstrate economic potential. To be sure, it is difficult to obtain comparable data for a number of different countries for long periods. It is not impossible though, and the utilization of inappropriate data remains a poor practice, especially if the figures selected for a particular span fail to reveal important changes over a longer period.

The organization of the book does not make for either easy or interesting reading. The material on each issue area (generally a chapter) is organized, first of all, under rather loosely defined conceptual categories ("jointness," "reciprocity," "specificity," "distance," "cumulation and spillover," "channel of communication," "administrative treatment versus politicization," and "domestic politics") and then, under each category, according to middle power. The lack of connections between the discrete historical vignettes under the innumerable headings is accentuated by the fact that the relationships among the concepts themselves are never explained. Although they are treated as if they are all in some sense equal and stand alone, such is clearly not the case (e.g., "reciprocity" apparently refers to a type of interstate activity, "spillover" to linkages among different activities, and "distance" and "domestic politics" to factors that might explain these activities). The generality of the concepts also seems to encourage some curious categorizing: for example, Canadian and American actions regarding Chinese representation in the UN are discussed under "jointness," the Canadian Development Corporation's attempted (unilateral) takeover of Texas Gulf is discussed under "reciprocity," and so forth. On the whole, the book seems much more concerned with categorizing historical events under these labels than with theorizing about them. The diverse phenomena for which often incidental explanations are offered include an array of formal government activities (for all five countries), various aspects of intragovernment decision making and of informal transborder linkages, as well as official and unofficial perceptions: it is thus difficult to detect a consistent theoretical thread.

The book's general conclusions appear to be that "constantly increasing attraction" characterizes all four countries' relationships with the U.S. and that "the superpower was not always the actor that had the greater influence" (p. 265). Unfortunately, the former conclusion, to the extent it can be evaluated, seems rather convincingly refuted by the evidence presented in the book itself—at least for Canada. It is never clear what the central notion of "atrraction" means, whether it pertains simply to "linkages" between countries, or to what might better be called "salience," or to the extent of "pleasurable emotions" (as the dictionary would suggest), or, in a more mystical sense, to being "drawn toward" each other. At any rate, the discussion in earlier chapters showing "declining responsiveness" in defense and monetary relations, greater national controls over immigration and foreign investment, and "more divisive" relations on environmental matters would suggest that "attraction" in most senses has been declining, not increasing. The consensus of Canadian foreign policy analysts would similarly be that changes over the 1960s and 1970s have resulted in a less "special" relationship with the U.S.

As to the second conclusion, it is hardly a stout affirmation of the "small-is-powerful" thesis to assert merely that the U.S. did not *always* have greater influence or that "mutuality of advantage was not ruled out" (p. 290). The important theoretical question is not whether the lesser power *ever* had equivalent influence or ever gained equal benefits on particular issues, no matter how infrequent; rather, the question surely concerns what factors determine not only the resolution but also the emergence and handling of *most* issues. There is very little in *The Politics of Attraction* to refute the conventional wisdom that preponderance of American power remains the dominant factor in the relationships with these middle powers. Largely ignoring the impact on the lesser state of a substantial power difference, and emphasizing instead its minor deviations within the narrow parameters set by this difference, does not make the central reality of superpower-middle power relations disappear. To assert that Canada was more independent of the U.S. on certain cold war issues than other middle powers does not refute the argument that deviations were the exception, not the rule. It is perhaps ironic that some American students of international politics have come to emphasize the influence of lesser allies at a time when the long-standing orthodoxy among Canadian analysts to the effect that the lesser state gained influence with the superpower through cooperativeness and cold war alliance is increasingly being challenged.

The weaknesses of this book underscore an important analytical point, namely, that the sort of multi-case comparison attempted here, no less than other approaches, has inherent limitations and pitfalls which even if recognized are not easily surmounted.

DON MUNTON

Dalhousie University

On Capitalist Underdevelopment. By André Gunder Frank. (Bombay: Oxford University Press, 1975. Pp. x + 113. $1.90, paper.)

This short book—really an extended essay—was written in 1963. A preface was added in 1969, and another in 1971. The author offers the essay as an "early statement" of "dependency theory" and an application of this "approach" to a series of problems. Oddly, however, he also says in the 1971 preface that "the 'dependence theory' has already come to a halt after having given all it has to give" (p. ix). What this latter statement means, and why one should care about a "theory" that he claims is exhausted, are questions which Frank does not really address.

The basic thesis of the work will be familiar to readers of Frank's other writings. It is that both underdevelopment and development are "inevitable" (p. 104) products of capitalism. Development and underdevelopment are nowhere defined clearly; Frank uses various indicators which sometimes are inconsistent with one another. Capitalism is everything that has happened in the nonsocialist world for at least the last four centuries; thus capitalism is "a single world-wide economic and social system" and includes not only capitalist "traits" (market relations, proletarianization, etc.) but also "feudalism, mercantilism, colonialism, etc." (pp. 44–45).

Among the specific arguments—some definitional, some empirical, some prescriptive—that Frank makes are the following: "underdevelopment and colonialism are perfectly correlated in both directions" (p. 7); "internal colonialism" is the intranational expression of international imperialism; stratification systems are multiple but classes are always dichotomous; "the proletariat . . . most certainly includes the peasants, 'subsistence' peasants included" (p. 94); "only the proletariat—and those already liberated by socialism"—can "liberate underdeveloped countries" (p. 105); *in only one country*—the Soviet Union—have "imperialism-colonialism and underdevelopment, both of the USSR and of its regions and peoples, . . . by general agreement been eliminated" (p. 103); "if rivers of blood are necessary for liberation from capitalism and underdevelopment now, oceans of blood may be necessary" in the future (p. 110). The book has no index, but there is a short bibliography.

On Capitalist Underdevelopment makes a clear statement of the dependency approach (or theory or perspective or whatever one chooses to call it) in one of its most extreme versions. In this it is comparable to the widely cited essays by Susanne Jonas Bodenheimer and by Ronald Chilcote and Joel Edelstein. The book is a convenient source of André Gunder Frank's main ideas. If it really is available in the United States for the listed price (all the way from Bombay?), it is also an inexpensive source. These are the book's main utilities for scholars and teachers. However, it adds no new ideas or data to the dependency literature. Scientifically, the work suffers from grave defects. The overall paradigm is a huge tautology: it says that capitalism *produces* underdevelopment, but it also links capitalism and underdevelopment *by definition* (for instance, see pp. 45, 51, 75). Frank simply ignores most evidence that does not fit his own "thesis" (which is often not a thesis but a definition). He repeatedly makes sweeping generalizations in the face of abundant contrary evidence and repeatedly uses empirical criteria in ad hoc ways, using them when the data fit his views, ignoring them when they do not.

A few of Frank's ideas are plausible and researchable, but most are simplistic, dogmatic, and tautological. Although Frank is more extreme in many respects than other radical and dependency writers, the basic model he presents has considerable appeal today in both academic and other circles. That so problematic a "theory" could become so widely and uncritically accepted in such a short time in the United States and elsewhere is distressing—at least it should be distressing to scholars. At the same time, it is sociologically interesting. Indeed, it may be just as important to explore the social, cultural and institutional bases of this appeal as to examine the substantive questions raised by the paradigm itself.

ROBERT A. PACKENHAM

Stanford University

Nuclear Weapons and World Politics: Alternatives for the Future. By David C. Gompert, Michael Mandelbaum, Richard L. Garwin, and John H. Barton. (New York: McGraw-Hill, 1977. Pp. xiv + 370. $6.95, paper.)

This volume is the result of one of more than 20 projected studies conducted for the 1980s Project of the Council on Foreign Relations. If it is an accurate indication, the series will provide sober and comprehensive coverage and material for serious discussion about the major foreign policy issues of the next decades; as one might expect, these studies seem less likely to succeed in producing clear-cut answers to difficult questions of policy.

The authors, a combination of scholars and government officials whose work involves nuclear issues, approach their subject by positing four different international nuclear regimes for the future, defined as systems of international obligations, national force structures and doctrines "that together govern the role of nuclear weapons in war, peace, and diplomacy" (p. 6). Their intent is to integrate technical and political considerations, and to relate both to larger international realities. Their success in this commendable enterprise is uneven, but given the tunnel vision which characterizes much of the arms control and nuclear strategy literature, clearly theirs is a sensible and needed approach.

The dilemma of thinking about the nuclear future, as Gompert correctly notes in his introductory essay, is that "because of the apparent weight of nuclear weapons in world security and world politics, the thought of dramatic change in the management of these arms summons no less anxiety than the expectation that present conditions will persist" (p. 5). This perplexing fact shows up clearly in the development of the four regimes, which are based respectively on these central themes: (1) continuation of the current regime of relative stability and great power dominance; (2) greater and more innovative arms control measures with the goal of reducing dependence upon nuclear weapons; (3) achieving an admittedly utopian "denuclearized" world, the paradox of which is that it is most likely to come about only after the holocaust; and (4) strategic deterioration through proliferation, mutually improved first-strike capabilities, or strategic imbalance. This last regime is in essence "a First Regime gone bad" (p. 310).

Taken together, these four regimes underscore the fact that stability and balance must be the highest priority policy concerns for the future, all the more so because of the importance of perception in making the root concept of deterrence workable, and thus in avoiding catastrophe. A stable nuclear distribution and technology may be the only way that perceptions can be kept in line; proliferation, the fear that the other side may be trying to achieve a first-strike capability, or fear of falling behind, could all be disastrous, in that each would be likely to alter perceptions in unsettling and unpredictable ways—even if assured destruction capability still existed for each major nuclear power.

This is the problem which the four regimes effectively illustrate. Only the first two are based on what in any sense could be considered a stable situation; and even for the second, the transition to lower levels of nuclear armament (including forswearing of tactical nuclear forces) could upset the balance and thus result in precisely what is was hoped to prevent. The transition from the current state to the third, or non-nuclear regime, whether in the nationalist or "incremental" mode or the internationalist "beyond the nation-state" version (both alternatives are discussed), would be dramatically unstable, as the author indicates. Furthermore, the kinds of international regulatory mechanisms envisaged would hardly guarantee that the end result would remain a system at rest.

All this would seem to argue for maintaining the status quo as a conscious policy choice. Unfortunately, as the quite credible fourth regime suggests, political change and technical advancement could make it impossible to retain the existing, if precarious, stability. In short, there is no guarantee that any of the three "desirable" regimes could work. This is the formidable difficulty in approaching the nuclear future.

In their efforts to outline models which together capture the whole range of possible futures, the authors have produced a framework which is only partially satisfying. They have excluded a regime based upon the development of defensive superiority over offensive nuclear capability on grounds that this is not now technically foreseeable, and not likely to be (p. 307), but have presented a non-nuclear regime which seems to me at least as unfeasible politically. If the purpose of the book is to foster thorough consideration of the future role of nuclear weapons, then it would have been helpful to develop the missing "fifth regime." If the purpose of the book, however, is to refine realistic policy alternatives, then perhaps too many regimes are included, rather than too few. Only the first and second regimes, as the authors recognize, represent real, current policy choices. The format of the book, which forces us to pay equal attention to the less "actionable" third and fourth regimes, makes the critical differences between the first two less distinct than they would be with more detailed examination.

There is also a problem, common in collective enterprises, especially ones carried out by those of the "model-building" persuasion in the study of international politics, of lack of parallelism in presentation. A graphic or chart summary of the four regimes, using identical variables or dimensions, would be an important aid to the reader.

These are minor criticisms, however. As a whole, this volume is a thoughtful and timely guide to the complexities which must be faced if the nuclear era is to remain under control. The book also contains a useful appendix by Franklin C. Miller, synthesizing the current global nuclear weapons situation in terms of strategic concepts, types and locations of forces, and arms control history, in a presentation which is remarkably complete for a short summary.

WILLIAM I. BACCHUS

Washington, D.C.

China, Oil and Asia: Conflict Ahead? By Selig S. Harrison. (New York: Columbia University Press, 1977. Pp. xvi + 317. $10.95.)

If Selig Harrison is right, oil will be the controlling factor in the evolution of power politics in East Asia for the foreseeable future. His provocative, far-sighted and carefully reasoned book compels readers to examine new dimensions of national interest, power, diplomacy and potential conflicts in this region. Without saying so, Harrison has produced a study which convincingly demonstrates the relevance of the principles of political economy to power politics—a refreshing and rewarding treat as compared to the outdated and arid cold war literature which continues to clutter up everyone's bookshelves. In a word, it is one of the few important works on the real fundamentals shaping Asia to appear in the last decade.

Harrison's basic thesis is that China, for strategic and economic reasons, must develop the potentially immense offshore oil deposits of the East Asian continental shelf. China must do this, the author argues, because its onshore oil will not be adequate to meet industrial needs and the location of this oil makes it militarily vulnerable. Because the resulting outreach for oil involves the competing claims and interests of Korea, Japan, Taiwan, the Philippines, Vietnam and, of course, the United States, this process is inevitably transforming the political calculus of the powers and, therefore, the very nature of their present relationships.

The argument supporting this thesis is roughly as follows: China's onshore oil reserves (according to Western oil authorities and the CIA) are insufficient for its long-range development goals because (a) though numerous, they are scattered, small or too deep beneath the surface for efficient recovery and (b) the most promising large reserves in the northwest are perilously close to the Soviet Union. On the other hand, development of the more extensive offshore oil would place Peking in a commanding position vis-à-vis its neighbors, and prevent the Soviet Union or other powers from controlling vital sea lanes (especially the Spratley and Paracel Islands) linking East Asia to the Indian Ocean. It follows that no power can safely proceed to recover the oil in the continental shelf without negotiations with China. According to Harrison, Peking has adopted a purposely vague posture about the extent of China's claims and the type of ocean boundary lines it might accept in this region pending the acquisition of a technological base which will enable China to play the strongest possible hand in the eventual settlement of these issues. In the meantime, China apparently condones "exploration" for oil on the offshore shelf, but not "development," by other interested parties.

While the obvious and most rapid way for China to exploit the offshore oil would be through joint ventures, lease arrangements or the wholesale import of Western know-how, Harrison believes that China will reject this course of action because it would make it dependent on foreign powers. Instead, he foresees a patient Chinese strategy of gradually acquiring the competence to produce the oil itself and by selective imports of Western technology. The vital importance of the offshore oil to the economies of China's neighbors, and therefore their independence, makes for a variety of potential conflict situations, more so regarding Korea, Taiwan and Vietnam than Japan, the Philippines and the United States. Of course, the other side of the coin, as Harrison points out, is that all these countries will also have strong incentives to work out mutually advantageous arrangements with China. Thus, Harrison reports, even some of the Taiwan officials he interviewed were aware of the possibility that the offshore oil might one day be a bridge to accommodation, though it has so far represented a bone of contention. Bearing this in mind, the author concludes with the sound advice that the United States should clearly dissociate itself from any Taiwan plans to grant oil concession zones more than 50 miles off the island, so as to avoid the implication that America's interest in the oil might be a symptom of lingering attachment to the "two Chinas" policy.

Such a bold and persuasively argued thesis necessarily rests on broad assumptions, some of which are open to question: (1) Estimates of China's onshore oil reserves, which are based on experience in the U.S. and the Middle East, may be highly inaccurate. Relatively little is known in the West about the oil potentialities of China's geological structure. The Chinese geologists think their onshore reserves are very great. Maybe they know something we don't.

(2) The vulnerability of China's reserves in the northwest probably will not be a deterrent to their full exploitation. In the nuclear age no oil field is safe—if one pushes the military argument to its logical conclusion. Moreover, for the most effective use in decentralized economic development (or in case of war, for that matter) the onshore oil must also be recovered.

(3) Harrison's case for assuming that China will play down the importance of coal and hydroelectric energy sources, which are very great, is not convincing. The very principle of "self-reliance," which he rightly emphasizes, will force China (as it has forced the U.S. and the USSR) to follow a balanced energy development policy so as to avoid the error of over-reliance on any one resource. This consideration is likely to weigh more heavily in Peking's decisions about coal and hydroelectric power than purely cost-effectiveness criteria.

None of the above qualifications seriously undermines the essential thrust of Harrison's argument. Rather, they imply only that even if he is right about China's oil "game plan," it is not the sine qua non, but a complementary aspect of its overall development and diplomatic strategies.

DAVID MOZINGO

Cornell University

Global Fracture: The New International Economic Order. By Michael Hudson. (New York: Harper & Row, 1977. Pp. viii + 296. $12.50.)

Global Fracture chronicles the momentous, sometimes crippling, shocks to the international economy during the 1970s and offers a provocative interpretation of the world they have wrought. Writing in a lucid, straightforward style, Hudson argues that the old order, based on U.S. dominance in economics and politics, finally collapsed in 1973—the year of generalized floating, the oil embargo, and America's export embargo on agricultural products. The new order, he believes, is a bloc system uniting Europe with the underdeveloped states of Asia and Africa. It is this union, not the demands of UNCTAD, that Hudson unconventionally terms the New International Economic Order. This new order "connotes a movement by Europe and Third World countries both to become independent of the U.S. economic orbit and more closely integrated economically and politically with one another" (p. 1). "The resulting diplomacy," he says, "may leave the United States isolated within its own hemisphere" (p. vii).

Skimming over serious problems of evidence and inference, Hudson simply treats the new "bloc system" as a compelling fact. He concentrates instead on a muckraking exposé of U.S. foreign economic policy. He is especially critical of U.S. balance-of-payments deficits in the late 1960s and early 1970s. Because of the dollar's unique role as a reserve currency (held by central banks to settle their international accounts), the U.S. escaped the ordinary restraints on its deficits. Its military expenditures, foreign investments, and excessive imports were financed by the foreign accumulation of dollars, creating excess liquidity for self-serving ends.

Hudson is equally critical of U.S. trade policy, charging that the U.S. pursued a nationalist course while imposing internationalism on its partners. Again referring to 1973, Hudson claims that "the United States reverted to economic protectionism. . . . It closed its doors to world investors and tied its foreign aid to strictly self-serving objectives" (p. 50).

Such hyperbole obscures more than it clarifies. Certainly protectionist pressures have been intense, and not only in the United States. But to say that the U.S. has already reverted to protectionism not only grossly overstates the case, it also forecloses serious study of the opposing trade coalitions and the policy disputes that have characterized the Tokyo Round of GATT negotiations. Moreover, it is simply not true that the U.S. has closed its doors to foreign capital.

If Hudson sometimes misstates U.S. policies and ignores their domestic and bureaucratic sources, he also disregards their international constraints. Consider the enormous U.S. payments deficits, which have at times eroded confidence in the dollar as a reserve currency. Here Hudson's arguments have several significant gaps. First, he fails to analyze the basic asymmetries defining the Bretton Woods system—asymmetries which became untenable in the late 1960s. For example, the dollar's crucial role was based in part on the United States' *unique* commitment to exchange its currency for gold. Second, there is almost no examination of the German and Japanese surpluses that were counterparts of the U.S. deficits. These surpluses were fundamental features of export-oriented economies, making exchange realignments extremely difficult. Rigid domestic policies like these compounded the problems of fixed exchange rates. Naturally, both surplus and deficit states tried to shift the burdens of adjustment, creating recurrent and nearly intractable problems of international negotiation. Finally, even though the U.S. refused to choke off its deficits, its policy makers did acknowledge some costs. (Hudson, by contrast, treats the deficits as undiluted benefits to the U.S.) Clearly, the U.S. did enjoy some seignorage benefits since it printed the world's money. And the persistent deficits did allow the U.S. to consume beyond its income. But a more complete treatment would recognize that the continually overvalued dollar seriously affected U.S. exports and employment, contributing to protectionist pressures. These problems, plus slipping confidence in the dollar, stimulated U.S. efforts to stem its deficits. Besides limiting tourism and foreign investment, the U.S. repeatedly tried to force revaluation on surplus countries. While these policies might be criticized, they are wholly incomprehensible within Hudson's framework.

Similarly, Hudson's depiction of the new bloc arrangements is overdrawn and oversimple. The unity between Europe and its former colonies is more tentative and problematic than he suggests, as is the movement toward producer cartels. By exaggerating existing bloc formations, Hudson slights more interesting and complex questions about their limitations and their future, particularly the uncertain relationship between the multilateral GATT system and the EEC's preferential trade policies. In this question, as in so many others, *Global Fracture* states its case in bold language but fails to provide systematic or subtle inquiry.

CHARLES LIPSON

University of Chicago

Marine Policy and the Coastal Community. Edited by Douglas M. Johnston. (New York: St. Martin's Press, 1976. Pp. 338. $18.95.)

Maltese Ambassador Arvid Pardo's eloquent plea for international management of ocean resources has sparked considerable political and scholarly interest. This volume, edited by Douglas M. Johnston, is a part of the new literature which has developed since Pardo's 1967 initiative.

The international community has struggled with relatively little success to bring about agreement concerning the distribution and use of the ocean's riches. The annual United Nation's Law of the Sea Conference, now in its third evolution, and thus called UNCLOS III, has highlighted and defined the differences among nations over the use of the oceans but has made precious little progress toward an international ocean regime.

Johnston's reader, like the Law of the Sea conferences, begins with the purpose of bringing order and form to a chaotic subject. The table of contents makes it appear that the book is well organized into logical topics concerning the economic background of ocean politics, the political interactions associated with international debate on the topic and, finally, the impact of the coastal zones on ocean political management.

In reality, the book simply breaks down into 13 individual essays with very little continuity among them. Of course, many of these essays do stand well on their own; nevertheless one must question the continuing need for this type of reader which really is nothing more than an elaborate journal.

The essays provide much information. Analysis is thin, however. Edgar Gold's essay opens the book with a competent overview and ample legal history associated with the perturbations of the LOS conference, but not until the seventh essay does the reader find a truly analytical discussion explaining the failures of UNCLOS III. Edward Miles gives far and away the most resourceful and satisfying contribution by taking the reader through a step-by-step review of the pertinent variables and trying to explain where and why they add up to failure. He relates stages of the LOS to political realities existing beyond the United Nations. Political groups and maneuvers within the UN are well documented and compared.

The book closes with a section on the coastal community which offers some interesting histories of selected areas. Here the reader sees in local areas the same kinds of conflict that appear globally—competing economic interests and problems of political management seem uppermost. As a group of separate essays, this section is interesting; as a part of a reader that ostensibly tries to tie global maritime problems with coastal issues, it is less satisfying. While a student of the Indian Ocean may find the problems of "modernization of peasant fisheries in Sri Lanka" a fascinating topic, author Paul Alexander does little more than stab at linking this essay to the whole. The same holds true for essays about Newfoundland fisheries and problems of small island societies.

Johnston does hint at the role of the coastal zone in the broader maritime context in the final chapter, but the analysis is frankly pale in comparison with the thorough job done earlier by Miles. And, while Miles covered coastal zone issues only incidentally in his analysis of UN action, one nevertheless gains the impression that he has explained this interaction section 3 far better than anyone in the fourth and final section of the book, because he has tied the intimate details of the UN conflict to global issues.

It is this failure to properly link UNCLOS III with world political problems that plagues the perspective of this book. Johnston's final essay, as point of focus, identifies the two areas of developing consensus pertaining to the LOS conferences but does not place them in context. UNCLOS III is leading to a universal extension of the 200-mile economic zone. A second theme, the creation of an international seabed authority, is far less substantial. What this means in plain language is that the nations of the UN have not reached any real agreement to internationalize the oceans but have only agreed to balkanize a large part of it. This is a consequence of national interests dominating international affairs and underscores the essential failure of the UN to achieve anything more than polite talk on the problem.

Johnston does argue effectively that the success of UNCLOS III is closely related to the problems of management. The key problem, unfortunately, is that the participating states have not yet determined what needs to be managed, by whom, and according to what standards. In short: business as usual.

DONALD E. MILSTEN

University of Maryland, Baltimore County

Law and Politics in China's Foreign Trade. Edited by Victor H. Li. (Seattle: University of Washington Press, 1977. Pp. xx + 467. $20.00.)

This collection of essays is of a generally high order of scholarship and should attract a commercial as well as an academic audience. But it is not free of limitations, and it may be helpful to identify them first.

Extended portions of the book, especially those relating to technical aspects of the organization and practice of China's trade, generally will not interest political scientists. Several of the contributions are considerably dated. The editor provides an updating and synthesizing chapter as well as his own study of the Chinese foreign trade decision-making process, but acknowledges that the collection lacks a guiding or conceptual framework. There are both country and functionally organized essays, but no special attention is given to such themes as China's trade with less developed countries, or to the foreign trade/foreign aid nexus.

Read together, the essays underscore the indivisibility of China's politics and trade. While this is an unexceptional conclusion, the detail in the essays discloses a distinctively Chinese but not necessarily antithetical interplay between ideological, cultural and other constraints or rigidities, and more cost/benefit-oriented considerations. Together they shape China's trade, and help to explain its political behavior more widely.

A recurrent theme is China's striving for self-reliance, and the fashioning of distinctive developmental programs. Randle Edwards comments that this is not new to the present regime, given such factors as China's historical geopolitical position, a conscious isolation that has stressed self-reliance, and a dread of external subjugation and of corrupting influences. Various contributors suggest how these characteristics have been manifested in China's commercial life. The Chinese are averse to litigation or to third-party arbitration as means for resolving contractual disputes. They stress contractual detail and standardization, as may be illustrated by their persistent suspicion of the legal tricks that, according to Frank Münzel, they ascribe to the "typical consequences of the turmoil and unreliability of capitalism." Jerome Cohen goes on to explain that they have exhibited uncommon suspicion of foreign commercial visitors, and at times have been sharply punitive toward them. Their hesitation to adapt to western standards of product design, packaging and labeling has in part been caused by their own anxiety about bourgeois contamination, and such adjustments were halted entirely during a phase of the Cultural Revolution. In an important essay on commercial relations with the Soviets, George Ginsburgs explains how, after strain had already developed between the two nations, a Soviet offer of grains was refused, and Peking elected instead to divert scarce foreign exchange toward purchases from the West. All the same, Ginsburgs reminds us that China's shift away from Soviet trade was inspired by practical considerations. China not only wished to avoid overdependence on a single source of supply, but increasingly reasoned that its own developmental priorities called for stress on agriculture and a more selective approach to industrialization—priorities and techniques that were belittled in Moscow.

China's trade with western nations has certainly carried political inspiration, but the consensus of contributors to this volume is that, on balance, it has been productive to Peking, both commercially and diplomatically. In Hong Kong, China has built up an impressive and valuable network of essentially capitalist structures. It was willing to place commercial officers in countries that had not yet recognized it, and subsequent improvement in trade may in certain cases have accelerated its diplomatic recognition, and the eventual erosion of its containment. In their survey of Sino-Japanese relations, Dan Henderson and Tasuku Matsuo recount the familiar Chinese inducement of the Japanese to undertake government-to-government contacts through the memorandum trade, to declare support for China's foreign policy objectives—while China was willing to play along with the charade of dummy firms that met its political criteria. Eager for trade with China, Japan found itself walking a narrow tightrope. Economic pressures on Tokyo hastened a normalization of Sino-Japanese economic and diplomatic relations.

China employed a variety of material and symbolic manipulations in its commercial relationships with Japan, but was considerably less stringent with West Germany. Here Arthur Stahnke's speculations are useful in appreciating why differing political rationales contributed to case-by-case Chinese conduct. He suggests that in addition to various economic considerations, Germany was not an obstacle to any fundamental Chinese objective. It never recognized Taipei and was not part of nor had any serious strategic or diplomatic role in Asia, where Chinese regional interests lay.

China is hardly unique in applying noncommercial criteria to the conduct of foreign trade. Indeed, it for years was the target of international, and especially American, commercial ostracism. Even after the opening of trade and of quasi-diplomatic relations, as Stanley Lubman shows, various pieces of American legislation with cold war overtones inhibited freer commercial intercourse.

As of this writing, the Carter administration's slowness in normalizing relations has prompted a Chinese shift to Japanese and European buying when possible. A fresh political phase has emerged in China with the ascension of moderates such as Hua and Teng, with a redirection toward importation of foreign technologies. Stronger emphasis on technological development modifies the earlier, Maoist preoccupation with self-sufficiency. Whatever the specifics of how China's post-Maoist leadership operates, there is reason to believe that China's state-controlled economy, its foreign policy requirements and distinctive perceptual screens will continue to season commercial relationships. The material in Li's book, although dispersed, poorly conceptualized and omitting some obvious themes, nevertheless provides a basis for measuring and understanding such behavior, and will reward the patient reader.

HENRY S. ALBINSKI

Pennsylvania State University

The Reluctant Europeans: The Attitudes of the Nordic Countries towards European Integration. By Toivo Miljan. (Montreal: McGill-Queen's University Press, 1977. Pp. viii + 325. $13.50.)

This is not a very satisfying or successful book. The author, who was born in Estonia, brought up in Sweden, and who teaches now in Canada, has produced a detailed study based on extensive use of documentary sources in the Scandinavian languages. Thus, it promised to provide us with invaluable materials on a somewhat neglected and rather inaccessible subject for most English-speaking political scientists—the "Nordic countries' relations with an integrating Europe during the three decades since the Second World War" (p. vii). Unfortunately, Miljan provides us with a great hodgepodge of information and very little else.

Miljan is clearly most at home with historical narrative. The descriptive sections of his three chapters on the relationships of Norway, Denmark, Sweden and Finland with the European Community and the United Kingdom until 1961 are truly fascinating. Here is a wealth of information available nowhere else. Moreover, the chapters are carefully documented and plainly written. Similarly, his account of the Nordic countries' negotiations with the EEC provides us with much useful data hitherto available only in very scattered sources. It is when Miljan starts attempting to develop an analytical framework for his narrative that he falters.

The author claims in the preface to the book that, as far as the Nordic countries are concerned, "the demands of economic interdependence have been recognized, but have been placed within the primary demands of sovereign independence" (p. vii). However, nowhere do we find a careful explanation of what the author understands by "economic interdependence" in the Scandinavian and larger Western European context except for a laborious and overlong chapter on the structure of trade much better presented in tables in an appendix. Nor is it clear, when it comes to "sovereign independence," whether he is talking about legal sovereignty, security, neutrality or noninvolvement, since he is caught in the maze of his own false conceptualization. It is only in the last five pages that Miljan reluctantly admits that political and economic policies are interdependent.

It is difficult to determine why at intervals throughout the book Miljan flirts with confused and confusing attempts to give it a theoretical infrastructure. We are told, for example, that "in the case of the Nordic countries the security question may be expressed in a two-dimensional continuum" (p. 6), and, that "for comparative purposes the security policies of the four countries may be conceptualised in the form of three concentric circles" (p. 26). For Denmark, two charts illustrate the "three objectives and four orientations" that are claimed to be the features of Danish foreign policy during the past three decades (pp. 157–58), while for Norway we learn that the underlying framework for the referendum result" may "best be illustrated with the aid of a 3 x 3 matrix" (p. 191). This kind of rootless conceptualization which seems to come from nowhere, go nowhere and achieve nothing more than distract the reader, would best be left unsaid.

Miljan would have done better to explain to the reader familiar with the literature on regional integration what exactly he means by the "necessary and inevitable integration" of the Nordic countries "into the region of Europe that is represented by the European Community" (p. 3). Somehow, this seems to rest largely on the assumption that Europe falls quite naturally into two distinct groupings of states: the "core" states of the United Kingdom, France, West Germany and Italy, and the "peripheral" states of Austria, Finland, Sweden, Norway, Denmark and Switzerland. The same goes for Miljan's very flimsy discussion of popular, elite and parliamentary attitudes in the four Nordic countries. Since he does launch into an analysis of elite attitudes, particularly with respect to the Danish and Norwegian referenda, he would do well to inform the reader explicitly who composes the various elites and how they fit in to the decision-making structures.

Finally and unfortunately, Miljan seems reluctant to draw conclusions from the wealth of material he presents. Several of his chapters end quite abruptly. As a result, one comes to expect an elaborate concluding chapter. Instead, the ten pages of "summing-up" merely repeat what the author unconvincingly sets out in the introduction.

GLENDA G. ROSENTHAL

Columbia University

Nuclear Power: Issues and Choices. Report of the Nuclear Energy Policy Study Group. (Cambridge, Mass.: Ballinger Publishing Company, 1977. Pp. xvii + 418. $16.50, cloth; $6.95, paper.)

This book is the product of a prestigious panel, chaired by Spurgeon Kenny, Jr., supported by the Ford Foundation, and administered by the MITRE Corporation. It is an authoritative review of current nuclear power issues and can be strongly recommended to anyone who wishes a basic understanding of those issues. President Carter is said to have relied upon it in his decision to discourage nuclear fuel reprocessing.

Nevertheless, the book does show the limitations of its origin. The authors are primarily U.S. academics, uncommitted to either side in the nuclear debates. Although they gave substantial attention to the proliferation issue, the book slights both U.S. political questions and foreign interests in nuclear power.

Thus, for the authors, the nuclear power question does not require consideration of social or political responses to energy changes. Life-style questions are explicitly rejected (p. 15). And, the authors argue that "the same processes which have adjusted the economy to technological change should respond to energy scarcities without significant increases in unemployment" (p. 49). They assume continued economic growth; the economic uncertainties are therefore those of relative costs and resources availability. The choice is to be left to market forces that will probably produce a mix of coal and nuclear power, welcomed as "a useful hedge against uncertainties" (p. 4).

The technical uncertainties are presented openly and carefully. The environmental, health, and safety risks of nuclear power are fully developed and contrasted with those of coal. This analysis is excellent and points out that nuclear power might prove the safer choice. Our knowledge, however, will not improve rapidly: "the present climate of doubt about reactor safety will persist for at least a decade" (p. 242). For the authors, this climate of doubt about social costs (which might ultimately weigh for or against nuclear power) means that society has no basis for overriding the conclusions based on economic analysis (p. 17). The authors may be correct in their judgment that economic factors offer a wise decision-making criterion, but they make little effort to analyze the political and psychological factors that so often shape energy policy.

After the conclusion that the nation has domestic energy flexibility and can reasonably keep open both the coal and the nuclear options, the logic turns international. The need to decrease the risks of nuclear proliferation requires that the U.S. discourage the use of plutonium whether through recycling or through the breeder. This proposition is much less fully supported. There is little analysis of how long the diffusion of plutonium technology can be delayed or of how effectively U.S. international economic leverage and political example can slow that diffusion. The discussion in the international chapters is instead one of a series of suggestions for incremental improvements. Some of the points deserve the attention they will receive because of this book: the proposal for a cutoff of further production of fissionable materials (p. 291) and the possibility of enriched uranium stockpiles as the *reductio ad absurdum* of the energy security argument for national enrichment and reprocessing facilities (p. 373). Nevertheless, the deeper interactions between U.S. and foreign energy policies are generally ignored—the book says little about the extent to which large U.S. oil imports might support high oil prices and economically encourage the spread of nuclear power and the sale of conventional weapons to cover balances of payments. Little is said about the politics of the international nuclear industry. Nor are the international institutional arrangements for nuclear power treated adequately—these arrangements almost certainly require more than incremental reform; ways in which U.S. policy might encourage such reform deserve much more thought.

The book is intellectually conservative and ignores many of the political and global issues that underlie and give emotional life to the nuclear controversy. At the same time, it deals very competently with the middle-range technical and economic issues and presents a well-argued brief for going slow on plutonium. And there are many important and persuasive side points—the indications of the environmental risks of coal (pp. 210–11), the criticisms of the reactor safety evaluation process (pp. 238–29), the remark that much of the population risk from nuclear power derives from a few specific reactors located near cities (pp. 239–40). It is precisely the intellectual conservatism of the approach that helps make these points credible, and the book almost certainly defines a wise interim policy. Nevertheless, it would be very desirable to redo the analysis from a broader viewpoint. One could then recognize the energy-associated tensions between North and South and within the developed world. One could also recognize the practical political factors that are likely to help or hurt the world in responding economically to energy transitions and in building institutions to control nuclear power.

JOHN H. BARTON

Stanford University Law School

Microstates in World Affairs: Policy Problems and Options. By Elmer Plischke. (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1977. Pp. 153. $3.00, paper.)

Mini-Nations and Macro-Cooperation: The Caribbean and the South Pacific. By Herbert Corkran. (Washington, D.C.: North American International, 1976. Pp. 229. $10.00.)

Few areas of research in comparative foreign policy are as barren of systematic analysis as the study of microstate foreign policy and participation in world affairs. In the limited cases where comparative analysis of microstate behavior has been attempted, it has been based on broad generalizations and assumptions devoid of empirical content. Rare indeed is the study of microstate behavior which relates foreign policy outputs to independent systemic and internal structural and process variables; even rarer is the study which seeks to test hypotheses against hard empirical data. The result is a body of literature below the "pre-theorizing" stage of analysis, in which questions about the relationship between micro-size and participation in world affairs have received only peripheral attention, and in which the very concept "microstate" is called into question as an operational concept. Against this background, students of microstate and small-state behavior will welcome the recent contributions to the literature by Elmer Plischke and Herbert Corkran, but any high expectations of theoretical and empirical breakthroughs in the comparative analysis of microstate foreign policy will be only partially satisfied.

Elmer Plischke brings a long and distinguished career in the study of diplomacy and foreign relations to the analysis of microstate participation in world affairs. Plischke is concerned with determining facts about the low level of participation by microstates in diplomatic representation, their treaty making and involvement in multipartite conferences, their membership in the UN and other international organizations, the difficulties they experience, and the problems they create for other countries and the international community. Interwoven throughout the analysis is a concern for the policy options available to the U.S. and the international community in dealing with a possible community of over 300 states, over half of them microstates. While much of the book deals with issues familiar to the serious student of microstate behavior, especially problems of proliferation and membership in the UN, two contributions of the study are especially noteworthy. First, Plischke's analysis of the policy implications of microstate proliferation is unsurpassed in the existing literature. The central question posed by Plischke is "not whether peoples are entitled to manage their own affairs and enjoy political independence, but whether, irrespective of their size and ability to fulfill their responsibilities, they should also be entitled to sovereign equality in the affairs of the community of nations" (p. 9). This is a timely question, and the options proposed deserve the immediate attention of not only policy makers in the U.S., but those in the international community and existing and emerging microstates as well. Second, Plischke has amassed an invaluable data set on microstate participation for anyone engaged in, or contemplating, research on microstate behavior. In addition to numerous tables of comparative data for specific categories of participation, there is a comprehensive table for both the community of nations (Appendix A) and potential additions to the community of nations (Appendix B).

Plischke's book has its shortcomings, however. Of particular note is his restriction of microstates to a population ceiling of 300,000 (including a category of sub-microstates below 100,000). This definition is overly conservative; an upper limit of a million, for example, would have expanded the category of microstates from 16 to 32, a more realistic reflection of the size distribution of states. In addition, the economic dimension of micro-size is ignored as an independent variable explaining limited participation in world affairs. Would the results have been different if states were classified by GNP rather than by population? Is it the combination of limited human and economic resources that explains microstate behavior, or is it one variable dominant? From a theoretical perspective, Plischke fails to establish a strong relationship between micro-size and international participation. We do not know which of his findings are significant, since tests of significance were not performed; we do not know the strength of the relationship between micro-size and participation; and we do not know which size dimension is the most predictive of microstate behavior. These deficiencies notwithstanding, Plischke's compact and perceptive study stands alone as the single most informed source of information on microstate *political* participation available.

By contrast, Herbert Corkran's book on *Mini-Nations and Macro-Cooperation* has only limited value to the student of microstate behavior. Corkran all but ignores the theoretical literature on microstate behavior in favor of a theoretical perspective drawn from integration theory. Consequently, Corkran has produced an interesting and informative study of regional integration, without, however, relating his findings to the problems of microstates per se. Undertaken in the belief that "the exposition of anything which contributes to a better understanding of integration provides valuable data and relevant variables for further theory building" (p. 11), his study also aims "to develop insights which will help to identify the alternatives for association and integration with each other and with the world which are open to microstates in the modern world" (p. 12). He achieves the first objective with both skill and great attention to detail, the second receives only incidental attention.

Corkran's central focus is the role of colonial powers, both past and present, in the development of regional organizations and institutions, and the assumption of control of these instruments of integration by indigenous leaders. Consequently, Corkran is more concerned with process than with structure, and microstates are objects rather than the subjects of analysis. Although the work contains a wealth of historical information on the development of regional integration in the Caribbean and South Pacific, as well as valuable insights into the future prospects for integration in these regions, it has little theoretical value beyond its insights into the integrative process. With the exception of an effort to relate contemporary Caribbean integration to recent observations by Ernst Haas (pp. 77–78), no attempt is made to relate the findings to an existing framework in integration theory. Corkran is to be commended, however, for bringing together a large body of information heretofore unavailable in such accessible form to the student of regional integration.

The deficiencies of Corkran's study are both numerous and glaring. First, the concept "microstate" is not operationalized, and no attempt is made to build upon the existing body of literature directed to the study of microstate behavior. Second, the work suffers from a lack of comparative statistical data, not just in critical measures of regional and international transactions, but also in such basics as population, GNP, and area. Third, no attempt is made to isolate the influence of the size variable in the integrative experience of microstates, and the question of the implications for integration of micro-size is sidestepped altogether. Only in the concluding chapter does Corkran attempt to apply his findings to microstates in general, and here only in the form of five broad recommendations having universal application (pp. 207–15). While Corkran's book deserves wide attention for its historical content and insights into the process of regional integration, those concerned with the theoretical and empirical analysis of microstate behavior will be better served by Plischke's *Microstates in World Affairs.*

CHARLES E. PIRTLE

University of Pittsburgh

Decade of Decisions: American Policy Toward the Arab-Israeli Conflict, 1967–1976. By William B. Quandt. (Berkeley: University of California Press, 1977. Pp. viii + 313. $14.95, cloth; $4.95, paper.)

This is a highly competent yet somewhat unsatisfying book. William Quandt's broad theoretical and practical knowledge, analytical acuity, and instincts of wisdom are evident throughout his text. He tells us in authoritative detail what successive presidents and secretaries of state did, and why; and he offers succinct critical reviews of each stage of policy making. However, I, for one, was hoping for something more.

Quandt's chosen technique is to focus on the strategic perceptions and preoccupations of American leaders in moments of crisis: the 1967 war, the 1970 Jordanian conflict, the 1973 war, the shuttle diplomacy of Henry Kissinger thereafter. He explains at the outset that he considers this a better path to understanding American policy than to offer his own analysis of United States national interests, or to examine the impact of domestic lobbying or bureaucratic infighting on policy making.

This focus produces a wealth of valuable narrative reconstructing the shifting perspectives of the White House throughout the years in question. Quandt has read just about everything available in print, and in addition draws extensively on his records from his own service on the staff of the National Security Council before and during the 1973 war. He punctuates his narrative with occasional critical analyses of how the perceptions of presidents turned policy making in particular fateful directions. These sections are well done but rather brief and circumspect, and Quandt would perhaps have done well to give us less detail about the comings and goings of Kissinger and put more of himself into the book.

While concentrating on the perceptions and choices of presidents certainly helps us understand the past record of policy, it tends to press our attentions into an unduly narrow action-oriented framework and to preclude the development of a critical perspective of the background of cultural and political influences that shaped the presidential perceptions in the first place. Why were Johnson, Nixon, and Kissinger so preoccupied with superpower relations in the Middle East? Why did they assume that Israeli military preponderance would stabilize the region? Why did they underestimate the vitality of the Palestinian issue? What socializing processes, what domestic political incentives and constraints, have served to shape their images and to set the tone of public discussion?

In order to know more about the pressures that do or don't encourage a president to think seriously and act effectively in Middle Eastern matters, we need to learn much more about the domestic political process. The president does not talk only with his own aides, does not read only staff papers, and does not think only of the national interest.

Quandt devotes surprisingly little attention to the subject of domestic pressure groups and the role of the Israel lobby. His introductory chapter includes a useful but discreet and inconclusive ten-page overview of the subject, in which he observes, reasonably enough, that domestic pressures constrain but do not determine policy; but he then goes on pretty much to ignore the subject altogether in the main body of the book. While no doubt he is right that domestic politics do not invade the situation room in the White House during moments of high crisis, this observation fails to take note of the everpresent low-lying cloud of pressure on the administration, during more normal times, to avoid any actions, statements, or even thoughts likely to displease Isreal. To try to explain presidential policy perspectives and the record of American diplomacy over the years without reference to domestic pressures, is like describing a horse race without mentioning that the horse that came in third was suffering from a broken leg.

In conclusion, the "decade of decisions" of Quandt's title is really rather misleading. It might better be called a decade of paralysis. Not that no decisions were taken: in the pages of this very informative book we find frantic little decisions being made all the time, from Lyndon B. Johnson fiddling with his hot line to Kissinger scurrying around the Middle East, like a rat in a maze, with his aerial maps and disengagement proposals. For a whole decade the United States government was feverishly trying to keep its nose above water in a region where its influence was severely limited and its motives widely distrusted, seeking a path of survival amidst various dilemmas. The spectacle of a superpower so little able to define or fulfill its fundamental interests is a remarkable one; but if one is to remark on it, then one needs to dwell more substantially on the underlying nature of the difficulty, on the construction of the maze and the conditioning of the rat, than Quandt has done in this otherwise able and valuable work.

MALCOLM H. KERR

University of California, Los Angeles

The Foreign Policy of Modern Japan. Edited by Robert A. Scalapino. (Berkeley: University of California Press, 1977. Pp. 445. $21.50, cloth; $5.45, paper.)

It is rather astonishing that, in spite of Japan's standing as one of the world's preeminent economic powers, comparatively few books have been published on Japan's postwar foreign policy. The appearance of *The Foreign Policy of Modern Japan,* edited by Robert A. Scalapino, represents an important step toward filling this gaping void. The book consists of 13 essays, written by Japanese and American scholars, encompassing a wide, if somewhat disjointed, range of topics. It is a useful volume for specialists and students alike. Used in conjunction with recently published articles and monographs, *The Foreign Policy of Modern Japan* is the kind of book that can serve as a textbook or reference work for graduate and undergraduate courses. While it does not obviate the need for a more comprehensive narrative of Japan's postwar diplomacy, the anthology may be the most valuable single source for students of Japanese foreign policy so far available.

The book is organized into five sections. The first deals with decision making and the foreign policy processes, with articles on the Foreign Ministry, National Diet, and Japanese styles of negotiating by Haruhiro Fukui, Hans Baerwald, and Michael Blaker, respectively. The second section moves from general analysis to concrete case studies. Akio Watanabe, Gerald Curtis, and Sadako Ogato focus upon specific issues to illustrate how the foreign policy-making processes work in practice. The third section, perhaps the most relevant of all, is devoted to Japan's foreign economic policies. Masataka Kosaka, Chalmers Johnson, and Gary Saxonhouse contribute articles on Japan's economic behavior in the world economy. Section 4 is concerned with problems and questions of national security with articles by Makoto Momoi and Donald Hellmann. The fifth and final section ties together the volume with two broad overviews of Japan's foreign policy offered by Seizaburo Sato and Robert Scalapino.

In any anthology of this sort, there are bound to be problems of unevenness of quality, disjointedness in treatment, and gaps in coverage. The first two sections, speaking of gaps, would have been strengthened immeasurably if they had included empirical studies of the policy-making role of the political parties (especially the Liberal-Democratic Party) and of the national press, because these institutions are absolutely central to an understanding of policy making in Japan. As Japan has tended to place priority on economic interests in its postwar foreign policy, the section on economic behavior, as illuminating as it is, might have benefited from a few more articles on such pertinent subjects as Japan's chronic balance of trade problems, trade and overseas investments strategies, and ties of interdependence with industrialized and Third World nations. Also missing, regrettably, is a study of Japan's energy needs and policies, an area of supreme importance for the country's national security, and a matter of pressing concern to policy makers in Tokyo. Though too much cannot be asked of one volume, the usefulness of the book as a reference work might have been enhanced considerably had more attention been given to the scope of Japan's diplomatic ties with the superpowers, neighboring nations, the Middle East, the Communist bloc, and other areas.

But not all the problems are inherent to the nature of the book as an anthology. There are other deficiencies that could have been overcome. One of the book's most obvious shortcomings is its failure to focus sharply on a common set of empirical and theoretical questions. It strives mightily but not always successfully to combine policy-relevant analysis with traditional modes of scholarly research without the guidance of a common, unifying set of theoretical questions. Several germane hypotheses, consequently, were not tested or even raised.Nor were important questions addressed: Is there a way of measuring whether a country like Japan, without natural resources and dependent on foreign trade, is indeed more vulnerable to outside pressures, as most people assume? How applicable are the unitary rational actor, bureaucratic, organizational, and cognitive process models of decision making in Japan? What standardized criteria might be applied to assess Japan's postwar "performance" in foreign policy? How "rational" or "effective" has it been? While country-specific research without reference to broader comparative concerns can of course make valuable contributions, particularly in view of the still very bare bookshelves on Japanese foreign policy, future research in the field, if it is ever going to lead to cumulative knowledge and insights, will probably have to follow a broader strategy of comparative study, one that is more closely integrated with the ongoing research in comparative foreign policy.

DANIEL I. OKIMOTO

Stanford University

Sharing the World's Resources. By Oscar Schachter. (New York: Columbia University Press, 1977. Pp. ix + 172. $7.95.)

The thesis of this short book (originally the 1975 Rosenthal lectures at the Northwestern University Law School) is that there exists an increasing tendency in international decisions to apply standards of equity to the allocation of resources. This tendency does not stem from moral idealism, according to Schachter, as much as it stems from what he calls "the necessities of a common predicament." The argument goes something like this: governments are compelled to cooperate by the imbalances and deficits they face, and by the intensified competition that exists for a larger share of the world's wealth. To cooperate they must have a shared conception of a principle on the basis of which to distribute burdens and benefits. Increasingly, equity is this principle. "How that broad and vague ideal can be given determinate content [is] the principal focus of this book" (p. 143).

The book is divided into three parts. Part I, "International Equity and its Dilemmas," examines two specific manifestations of the principle of equity which Schachter takes to be particularly important: entitlements based on needs (of individuals, states as collectivities, and the planet as a physical and even social system), and historic entitlements. Entitlements based on needs rest on a valuational consideration, but needs to some extent can be determined objectively. Preferential treatment of poor countries is an example of the standard of needs-oriented equity at work, though Schachter waffles on the problem of potential and actual clashes between the needs of poor people in poor countries and the collective needs claimed by their governments. Historic entitlements are not valuational but formal. They derive from the legitimacy of the process of acquisition and transfer, irrespective of its substantive outcomes. They include the idea of sovereignty, rights of access to resources beyond national jurisdiction, customary rights and rights arising from consensual and contractual transactions. Particularly potent as an instrument of international change, Schachter feels, is the combination of these two principles, as when historic entitlements are claimed for the fulfillment of needs. His analysis of these principles is "empirical," that is, "not based on moral theory or philosophical premises but rather on the actual positions taken by governments in their claims and responses regarding the world's resources" (p. 23). Part II, "Sharing the Common Heritage," seeks to demonstrate the role of these principles of equity in the Law of the Sea negotiations, and in the international management of fresh-water resources and of the environment. Part III, "Equity in Distribution," similarly examines the idea of a "just price" for commodities, the need for greater transparency in and political control of technology transfers, the exercise of permanent sovereignty over natural resources and world food security.

Schachter is a distinguished theorist of international law and a seasoned practitioner of the craft of international organization. One therefore expects much from him. And there is much of merit in this book. Inter alia, as they like to say at the UN, Schachter recognizes that international debates and resolutions are neither decisive nor irrelevant, but that they can serve to delegitimize existing norms while engendering new expectations among actors about future distributional patterns; he distinguishes between altruism as a motive and equity as a product of decisions and transactions, and rightly maintains that the former is not necessary to produce the latter; he criticizes the assumption, found both in the literature and in the practice of international organization, that administrative procedures and networks of officials are somehow sufficient to resolve difficult issues independently of agreement on substantive criteria; he realizes that politically the most difficult environmental problems result from the cumulative effects of industrialization and not from such highly visible issues as aerosols and ozone; and the empirical materials he presents, particularly his discussion of international restrictive business practices in technology transfers, can serve as useful introductions for those who have not been following recent international negotiations on global issues.

But the book also has its shortcomings. They fall into three broad categories. The first is evidentiary. No matter how hard I try, I find it difficult to accept the empirical materials as evidence for the basic thesis of the book. I don't know how one has to look at the UNCLOS, for instance, especially the 200-mile economic zone, to see it as an illustration of the idea that standards of equity increasingly determine the allocation of resources—except perhaps in the cynical sense that the Latin Americans exploited the idea of equity to hoodwink the other developing countries, or in the ironic sense that the most global negotiations yet have resulted in an unprecedented extension of national jurisdiction, which in any case is by no means unfavorable to the industrialized countries. Nor is it clear that commodity price stabilization schemes will alter the relative position of developing countries in the international economic order. Moreover, the fact that there now exist almost 100 international joint commissions to manage waterways as opposed to one in 1815 proves nothing in itself except that we live in a world of exponential growth rates. As for changes in the distribution of technological capacity, they are few and largely reflect changes in the indigenous strength of some developing countries, owing little to international decisions. Lastly, the area of world food security may reflect greater equity, but it is difficult to argue, as Schachter's position would require one to argue, that what was once charity in this domain has now become a right or an entitlement. In short, *claims* for greater equity are abundant. The *use* of this universal principle to legitimate the pursuit of particularistic interests is even more so. Furthermore, *egalitarianism* in international decision making may be increasing in certain issue areas. But equity as a standard, let alone as an outcome, is a more dubious proposition.

The second shortcoming concerns the policy recommendations Schachter offers. He suggests a number of formulae, institutional mechanisms and procedures which would give more "determinate content," as he puts it, to the ideal of equity. The list contains many useful suggestions. But if standards of equity are not necessarily *contributions* to decision making but its possible *product*, then what we need are instrumentalities which would harmonize the diverse self-interests of governments in such a way that greater equity results from collective decisions. In other words, to be helpful, the policy recommendations ought to focus on the self-interest which goes into decision making and not presuppose and then elaborate on the very equity which may only be its unanticipated consequence.

Third, and notwithstanding Schachter's protestations to the contrary, there is a fundamental incompatibility between the two principles of entitlement which he advances: "needs" and "historic entitlements." He undoubtedly knows, though he does not acknowledge it, that he is right in the midst of a lively debate among moral and political philosophers on this very issue. The debate pits radical libertarians like Robert Nozick (*Anarchy, State and Utopia* [New York: Basic Books, 1974]) against neoclassical liberals like John Rawls (*A Theory of Justice* [Cambridge, Mass.: Harvard University Press, 1971]). The protagonists in this debate certainly believe that a contradiction exists between entitlements based on needs and fairness, on the one hand, and historical entitlements like the right to own, produce and exchange, on the other. Schachter "resolves" the contradiction by saying that governments link the two: "This is not merely a matter of "dialectics" or rhetoric. It reflects a political judgment that as an international criterion the maxim 'to each according to his need' is too impractical and far-reaching in its implications to win acceptance as a general principle; but when this maxim is conjoined with and limited by a principle of legitimacy, it becomes more acceptable because less threatening to the international order" (p. 20). Quite so. But building his case purely on the "empirics" of what governments say and claim leads Schachter to misread the evidence. First, to the extent that equity enters into these international decisions at all, it expresses the tension and contribution between the two types of entitlements, *not* some single standard. Schachter's own illustrations suggest contradictory tendencies, and he should have taken these more seriously than the deliberately obscurationist pronouncements of governments. Second, and again on Schachter's own description, there already exists a principle of allocation internationally; it may be a standard of equity but it has little to do with equity as he wants us to understand the term. To paraphrase the quotation above, those claims for changes in distributional patterns are successful which express the collective needs of governments without threatening the legitimacy of the existing international order. But this is not change. This is, as Richard Poirier once said about Norman Mailer, applying to new issues and circumstances methods which already have been worked to exhaustion.

Nevertheless, the book is timely, informative and suggestive, and introduces important considerations about the role of justice in emerging global issues.

JOHN GERARD RUGGLE

University of California, Berkeley
Royal Institute of International Affairs, London

War: Controlling Escalation. By Richard Smoke. (Cambridge, Mass.: Harvard University Press, 1977. Pp. xvi + 419. $18.50.)

Richard Smoke's book, a revision of the dissertation that won the 1971–72 Helen Dwight Reid Award, seeks to provide theory with operational relevance to officials. Thomas Schelling is the intellectual progenitor, and Robert Jervis' two books are the best recent companion pieces to his work. Smoke's *Controlling Escalation* is an extremely thoughtful, judiciously researched, carefully honed, and very valuable contribution to this effort. Those devoted to cognitive interpretations of foreign policy will find it particularly impressive. Those like me, who harbor reservations about psychological approaches—on the grounds that the analyst rarely has more right than the policy maker to assume the omniscience, objectivity, or comprehensiveness of perspective necessary to pinpoint errors in ends-means calculations that are avoidable systematically or a priori—will still find this study rewarding, but may have lingering doubts. This review emphasizes those doubts disproportionately and should not mislead the reader about the book's merits.

Smoke surveys five cases of success and failure in escalation control in limited war: the Spanish Civil War, the Austro-Prussian War, the Franco-Prussian War, the Crimean War, and the Seven Years' War. This approach is a refreshing antidote to the excessively technocratic and ahistorical tenor of so much of contemporary American writing on strategy. But it is still disappointing that Smoke avoided post-1945 cases entirely because data on the decision making of *both* sides in wars was available only for earlier conflicts. Yet he maintains, not entirely convincingly, that the lessons are applicable for contemporary policy makers despite the nuclear revolution. More extensive extrapolation of his conclusions, perhaps to a critique of current strategies of selective nuclear options and intra-war deterrence theories, would have been especially interesting.

Smoke sees an inherent tendency toward escalation in war and focuses on the "cyclical-sequence" variety, in which the action-reaction process feeds on itself, creates unexpected pressures for intensification or expansion of operations, and yields usually disastrous widening of war. He derives from his cases a rich menu of categorizations, causal patterns, and propositions that is not easily summarized. Beyond Schellingesque dissection of "saliencies," Smoke gives special significance to the initial context of conflicts, activation of latent objectives, interaction between escalation of stakes and tactics, and the need to be sensitive to an enemy's *expectations* more than its objectives. Perhaps most emphasized as sources of miscalculation are analytic failures and cognitive dysfunctions. Smoke sees the first as caused by "failures of imagination, failures of empathy, or failures of conceptualization" (p. 253), rectifiable through more careful, rigorous, and introspective calculation (a skeptic who assumes the fog of war will always be aggravated by extreme ignorance, uncertainty, secrecy, and deception will be dubious). The second leads Smoke to two "cognitive desiderata": to control escalation, one should think in terms of sequences, several moves into the future rather than just the immediate consequences of one click in the ratchet, and in terms of interactions, impacts on the enemy's expectations as well as on one's own objectives.

Some of my criticisms may be trivial or unfair (wondering why the author didn't write a different kind of book). In a few instances, Smoke indulges in definitional mystification, such as "nonreciprocal opportunistic escalation" (p. 58), by which he means "tactical initiative." He also embraces cognitive dissonance theory too uncritically in explaining the narrowing of the "field of expectation" (p. 284), and does not argue convincingly enough that the subjective narrowing is not matched by an objective narrowing of possibilities as escalation progresses. Smoke errs (though I have a vested interest in my own work, in making this criticism) in characterizing the dynamics of escalation in Vietnam in terms of the prevalent "quagmire" interpretation. An interesting approach Smoke might have taken would be to analyze escalation dynamics in different types of international systems (multipolar, bipolar, hierarchical; revolutionary, moderate, etc.) or between different dyads (strong state vs. weak state, strong vs. strong, etc.). Some of this sort of analysis is implicit in the cases, but would have been as interesting to schematize as some of the points he does belabor.

One problem is that it is difficult to see how Smoke's recapitulation of some cases is more than a terminological revision of the traditional analysis of balance-of-power statesmanship. In his discussion of the "Analytic Asymmetry" in the Franco-Prussian War (pp. 128 ff.), for instance, it seems that virtually *any* variable in the diplomatic postmortem relates to escalation control. In the Spanish case I am not convinced that escalation control as a conscious goal or orchestrated process was more determining than the simple political asymmetry of goals and commitments: Italy and Germany escalated because they were revisionist and aggressive; Britain did not because the Conservative government was anti-Republican; France did not because it feared provoking Germany without British support; and the Soviet Union did not because it was geographically malpositioned and could not afford to divert significant military resources. Escalation control was a result of the balance of power more than a determinant.

Finally, the reader may suspect that Smoke often tacitly identifies escalation control with *non*-escalation. For example he recommends conciliation, "even where there do not appear to be other reasons for doing so" (p. 293), for the sake of control. It is interesting that he cites the Spanish Civil War as a success in control (for that matter, why not the French non-response to the 1936 Rhineland remilitarization as well?). That the global conflict with fascism was deferred until 1939 *may* be fortunate, but this highlights an issue Smoke does not dwell on: the extent to which escalation control is a norm unto itself.

RICHARD K. BETTS

The Brookings Institution

Strategic Disarmament, Verification and National Security. By the Stockholm International Peace Research Institute. (London: Taylor and Francis Ltd., 1977. Distributed by Crane, Russak & Co., New York. Pp. 174. $16.50.)

For various reasons there has been a revival of scholarly writing concerned with evaluating a problematic matrix of theoretical contentions, concepts, and questions involving strategic disarmament, arms races, and nuclear proliferation. As an effort to shed new light on the role of verification within strategic disarmament, this brief volume certainly represents a part of that literature. Its main purpose is to explore the relationship between verification and the military security of nations involved in a simulated disarmament process.

The book falls into two sections. The objective of Chapters 1–5 is to define the concepts of "security" and "verification" and to emphasize their importance in the preparation and implementation of a disarmament agreement. Myriad conceptual terms and theoretical notions are introduced in this section as factors which might influence or condition verification and security. Their importance to the volume, however, is questionable. Despite the assertion that a disregard for these elements would have led to faulty inferences (p. 1), it is difficult to see how the *superficial* examination of such factors as the balance of power, deteriorating international relations, and public opinion adds to the reader's understanding of verification or national security. The balance of power and public opinion are each given a cursory examination, both of which together take up little more than a page. Moreover, the inclusion of these items contradicts the study's acknowledged attempt to delimit itself by avoiding political and economic analysis (p. 100). Thus, the discussions pertaining to these factors (especially chapters 1 and 3) could be deleted without damaging the central purpose of the investigation. The great strength of this part of the book is its excellent discussion of the various meanings of verification. Previous efforts to incorporate the concept of verification in disarmament proposals were undoubtedly plagued by the linguistic and psychological confusion that often linked verification with rigorous inspection and control. The study is quite correct when it suggests that in earlier disarmament negotiations, control implied much more than simple supervision or verifying that a nation was discharging its obligations in accordance with an agreement.

The second section, comprised of chapter 6 and the appendix, contains the substantive portion of the work, that is, an evaluation of the relationship between verification and military security. This evaluation is derived largely from a simplified, yet interesting, case study involving the simulated disarming of two nations. The technical considerations and assumptions of the exercise are thoroughly explained in the appendix. In general, the case study explores two scenarios: (1) a case of unextenuated disarmament involving no external factors; and (2) a situation where technological improvements are allowed to interact freely with the disarming process. Two elements of knowledge emerge from these investigations. The good news, we are told, is that verification should not be a strict requirement in the early stages of disarmament, provided that both parties to an agreement have a second-strike capability. The bad news is that a verification system, especially at the later stages of disarmament, cannot be restricted simply to the number of weapons in a strategic arsenal, but must be broadened in scope to include activities relating to the research and development of military technology. In fact, the study concludes, if verification is to play a meaningful role in disarmament, its *primary* task should be the confirmation of developments in military technology.

Judged on the basis of what it sets out to do and given its startling assumptions and constraints, this work is a generally acceptable and useful piece of research. Exercises of this type are often valuable in uncovering alternatives and implications which may otherwise remain confused or unnoticed. There is a point, however, at which they begin to lose their effectiveness. This is especially apparent when we are forced to relax the initial assumptions of such studies or attempt to apply their resulting conclusions to political and social reality.

For example, a crucial premise of the investigation, particularly if we are to accept its results, is that each nation has a secure and effective second-strike force that will respond when summoned. Based upon the United States' recent difficulties with the command and control dimension of its strategic system, this may not be such a tenable assumption.

Difficulties also arise when political and idiosyncratic factors are allowed to impinge upon the conclusions offered. If domestic politics have placed extensive limits on policy makers' options, then the *logic* of not requiring verification will not matter. Moreover, belief systems and images do not always yield to rational analysis. If decision makers *perceive* the need for strict verification at the early stages of disarmament, it may be difficult to convince them that they are analytically wrong.

In sum, the book is a reasonable and useful study of the role of verification in the disarmament process, but it should be read with an attentive awareness of its limitations.

LAWRENCE V. GOULD, JR.

Purdue University

The International Politics of the Nigerian Civil War, 1967–1970. By John J. Stremlau. (Princeton, N.J.: Princeton University Press, 1977. Pp. xix + 425. $26.50, cloth; $9.95, paper.)

The volume of literature on the Nigerian Civil War has grown substantially since the defeat of Colonel Ojukwu's forces in 1970. This literature ranges from journalistic recounting of events through emotional first-hand recollections to scholarly analysis by participants as well as external observers. By now it might be thought that every aspect of the struggle had been examined.

But John Stremlau's work is not just one more study of this tragic period in Nigeria's post-independence history. It is a closely researched and highly detailed examination of the international diplomacy that surrounded the attempt to break up the unity of Nigeria by creating a new independent state, Biafra. The most distinctive feature of the volume is the use of hitherto unavailable material drawn from a wide variety of sources, including the Biafran government's own records. Even more noteworthy is the author's use of interview materials. Not only was he able to gain access to many of the principals on both sides and in the international community, but also he was evidently able to establish sufficiently close rapport with them so that they permitted attribution of their comments in his book. The interviews reveal new facets of the Biafran struggle as well as illuminating the plans and motivation behind many acts and statements that have hitherto been left in question.

In his sketch of the political background leading to the announcement of Biafran secession on May 30, 1967, Stremlau stresses the important point that it was Gowon's creation of 12 states before that date which ensured the support of the minority tribes of the former eastern region for the federal cause, thereby fatally weakening the Biafran effort by isolating the Ibos. Although federal power to impose the new structure may have been doubtful, the recognition of minority rights ultimately contributed greatly to the federal victory and may well have made possible the continued development of the country as a unified whole.

Stremlau's excellent research serves as a forcible reminder of the extent to which the outside world became involved in the Biafran uprising. Early, and with considerable skill, the Biafran government sought foreign support. The appeal for arms became inextricably mixed with the enclave's urgent relief needs for food and medical supplies. The combination of the humanitarian desire to alleviate human suffering and the Biafran claims of genocide by the federal forces brought forth a worldwide emotional response in liberal circles that overshadowed the political realities of the situation to the point where international negotiations to restore peace to Nigeria were seriously impeded. The OAU's effort to support the internal unity and external boundaries of a member state became increasingly less effective as more non-African powers became interested in the fate of Biafra. The British government's strenuous diplomatic maneuvers were blunted by growing public pressures at home urging intervention to prevent further suffering. In retrospect, the American position of neutrality (with its implication that the problem was, after all, the major concern of the British) seems all the less tenable in view of our professed support for the inviolability of African boundaries. One need only recall, however, the deeply emotional appeals made on behalf of Biafra during the latter part of the struggle by U.S. humanitarian groups, to realize how difficult was the position of American policy makers at that time.

Stremlau deals with the rebellion with a remarkably even hand in his account of the main events, although it would appear that, on balance, his sympathies lie with the federal side. General Gowon emerges as a rather more attractive figure than he has been portrayed by other observers. Somewhat hesitant at first, he becomes more forceful as the war wears on. Convinced, as he was from the outset, that the federal task was primarily to put down a rebellion that, if successful, would spell the end of Nigeria as it existed at independence, Gowon became increasingly irritated at well-meaning attempts from outside to bring about a peace negotiated between equals—as he forcefully put it, he had no intention of negotiating with one of his military governors. The author makes clear, however, that on both sides the military and economic campaigns were carried on by competent and at times, even brilliant individuals who were proud of the cause for which they stood.

Apart from its other merits, Stremlau's book is marked by a clarity and grace of style only too rare in this period of methodological obscurantism. It will be enjoyed by anyone interested in post-independence Africa and who is concerned with the future of Nigeria. This volume may not be the last word on the crisis of nationhood created by the Biafran rebellion, but it will certainly rank among the soundest and most objective scholarship that has yet appeared on the subject.

L. GRAY COWAN

State University of New York, Albany

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